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ABSTRACT

This guide is intended to assist states and service delivery areas (SDAs) in addressing the new oversight responsibilities and opportunities stipulated by the Job Training Partnership Act (JTPA) with respect to implementing net impact evaluations. The following topics are covered in the individual chapters: the objectives and major provisions of JTPA and the importance of net impact studies to JTPA; the conceptual framework of a net impact evaluation (general research questions, participant outcomes and subgroups, and program activities and environmental conditions); research design (participant and comparison groups, selection of comparison groups, and sample sizes for participant and comparison samples); data collection and processing plans (selecting participant and comparison groups; obtaining preprogram data for each group; preparing a preprogram analysis file; and obtaining and processing JTPA program, outcome, and management information system data); data analysis plans (examining the adequacy of the comparison groups, estimating the average net impacts of JTPA programs, obtaining net impact estimates for various subgroups and by program length of stay, and adjusting for potential data and design deficiencies); cost analysis and benefit-cost comparisons; and relationships among the net impact, gross impact, and process analysis models. (MN)

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JTPA
EVALUATION
DESIGN PROJECT

# JTPA Evaluation at the State and Local Level

## Volume VI:
## An Implementation Manual for Net Impact Evaluations

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By Terry Johnson

March 1986

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# CONTEXT OF THIS VOLUME

This is one in a series of volumes produced by the JTPA EVALUATION DESIGN PROJECT.

## PURPOSE AND PHILOSOPHY

The purpose of this project has been to develop a set of evaluation tools that are useful to states and local service delivery areas (SDAs) in judging the way their JTPA programs are being managed and the impact they are having. The intention has been to base these analytic and managerial tools on sound program concepts and research methods, and to design them such that the information obtained is of practical and direct use in improving JTPA policies and programs at the state and local level. This kind of information is also expected to make a unique contribution to national training policy and Federal oversight of JTPA.

It is hoped that these volumes will stimulate and support state and local evaluation efforts in JTPA, and promote more consistency than in previous programs with respect to the issues studied and the methods used to investigate them. An important goal is to encourage the generation of complementary information on program implementation and impact that is comparable across states and SDAs. Comprehensive, comparable information is essential to the development of a valid and reliable knowledge base for resolving problems and improving programs. It is also required for adjusting national training strategies to changing needs and priorities at the state and local level.

## PRODUCTS

Consistent with this purpose and philosophy, the project has produced a set of materials to assist states and SDAs in evaluating their programs. These are to be useful in planning, designing and implementing evaluation activities. As an integrated collection, each set is developed to support comprehensive evaluations over the JTPA planning cycle.

The careful tailoring of these materials to state and local users is appropriate. JTPA represents a new employment and training policy shaped not only by the experience of managers and the perspectives of employers, but by scientific assessments of previous approaches for addressing unemployment, poverty and other barriers to economic security. In this context, the value of JTPA programs is also expected to be judged. In fact, the Act's assessment requirements are more explicit and sophisticated than those of any employment and training legislation to date. It clearly distinguishes between *monitoring* activities, whose purpose is to determine compliance (such as with performance standards) and *evaluation* activities, whose purpose is to determine how a program is being managed and implemented, and the kinds of effects it is having on recipients and relevant others. Equally significant, new constitutencies are expected to make these more rigorous assessments. States and SDAs now have this important responsibility. It is the first time in the history of employment and training programs that the Federal government's evaluation role has been significantly reduced.

This change affords states and local areas opportunities to influence public policy. It also requires them to assume new oversight responsibilities. Program evaluation is expected to become an integral part of the management of organizations administering, planning and delivering public training services. This is as it should be. The more information available at these levels, where changes in organizations can most readily be made, the more effective the management of JTPA programs. This project was undertaken in that context.

The evaluation tools produced by the project have been developed with a sensitivity to the differing needs, interests and resources of state and local users. They have been packaged into a single comprehensive and integrated set of volumes called *JTPA Evaluation at the State and Local Level*. The set contains planning and evaluation *guides* and *issue papers*. The following volumes are available in the set:

| Volume | Author |
|---|---|
| I: Overview | Project Team |
| II: A General Planning Guide | Deborah Feldman |
| III: A Guide for Process Evaluations | David Grembowski |
| III Supplement: Some Process Issues at the State Level | David Grembowski |
| IV: A Guide for Gross Impact Evaluations | Carl Simpson |
| V: A Guide for Net Impact Evaluations | Terry Johnson |
| VI: An Implementation Manual for Net Impact Evaluations | Terry Johnson |
| VII: Issues Related to Net Impact Evaluations | |
| A. Issues in Evaluating Costs and Benefits | Ernst Stromsdorfer |
| B. The Debate Over Experimental vs. Quasi-Experimental Approaches | Ann Blalock |
| VIII: MIS Issues in Evaluating JTPA | David Grembowski |

NOTE: Although each of the discrete products listed above is the responsibility of a single author, each seeks to incorporate the results of professional peer review, the many excellent recommendations of the advisory group, and the ideas and suggestions of the numerous practitioners interviewed in the process of developing these materials.

To further qualify these volumes, Volume III is accompanied by a supplement for state users. This is consistent with the significant differences between states and SDAs in the kinds of process issues that are most essential to study. The volume on net impact evaluations is sufficiently technical, because of the statistical methods involved, that a practical manual has been written to accompany it. This guide and manual tend to be more appropriate for states, since relatively large sample sizes are required for analysis. However, they are equally useful to larger SDAs and consortia of smaller SDAs which may want to jointly study the net impact of their programs. Regional evaluations, for example, can be very productive in providing management information relevant to regional labor markets. Although there is a separate issue paper on evaluating costs and benefits, this issue is also covered in the gross impact and net impact guides. In this respect, the user benefits from three related but different approaches to this important element of program evaluations. Also, the user should be aware that the Appendix of Volume II includes *A Report on a National/State Survey of Local JTPA Constituencies*. This survey was carried out by Bonnie Snedeker, with the assistance of Brian O'Sullivan, to provide additional input from practitioners to the development of the planning and process evaluation guides.

In conclusion, several expectations have directed the development of these volumes:

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## THE GUIDES

### The General Planning Guide

This guide is to assist users in *planning, funding and developing an organizational capacity* to carry out process, gross outcome, and net impact evaluations and to *utilize their results*. Separate state and local versions are available.

### The Evaluation Guides

These volumes are to have the following characteristics:

☐ The guides are to *complement* one another.

- They are to provide information on program management and other characteristics of program implementation, which can:
  - —Describe the way in which administrative, managerial and service delivery policies and practices operate to affect outcomes, as a set of interventions separate from the program's services.
  - —Pinpoint the source, nature and extent of errors and biases for which adjustments must be made in gross and net impact evaluations.
  - —Help explain the results of gross and net impact evaluations.
- They are to provide information on aggregate gross outcomes, and outcomes differentiated by type of service and type of recipient, which can:
  - —Describe relationships between certain implementation modes and service strategies, and a broad array of client and employer outcomes.
  - —Help explain the results of net impact evaluations.
  - —Suggest the more important outcomes that should be studied in net impact evaluations.
  - —Help sort out those aspects of implementation that may be most critical to study in process evaluations.
- They are to provide information on net impact (the program's return on investment), which can:
  - —Closely estimate the effect of the program's services on clients.
  - —Suggest which services and client groups are most important to study in broader but less rigorous gross impact studies.
  - —Help identify the decision points in program implementation (particularly service delivery) which may be most important to study in process evaluations.

☐ The guides are to enable the user to carry out *comprehensive assessments* of JTPA programs.

- They are to allow the user to acquire several different perspectives on the same program within a particular time period: on program implementation, on outcomes for clients and employers and on net impact.
- They are to permit the user to interrelate these different kinds of information to gain a wider understanding of what is happening in a program and why.

☐ The guides are to describe approaches and methodologies as consistently as possible, to achieve *comparability*.

- They are to define variables and relationships as similarly as possible.
- They are to define research designs, and methods of data collection and analysis using as similar concepts as possible.

☐ The guides are to *draw from past research* on employment and training programs, as well as seek *new* approaches and methods of specific value in evaluating JTPA at the state and local level.

- They are to replicate, to the extent possible and feasible, the issues and measures reflected in Federal monitoring and evaluation decisions.
- They are to make selective use of the results of relevant CETA studies, national studies of JTPA, and issue papers on JTPA evaluation by national public interest organizations in the employment and training area.
- They are to rely on the professional literature in applied social research.

## THE ISSUE PAPERS

Volume VII contains two issue papers which serve as companion pieces to the preceding volumes on net impact evaluation. The first paper on cost-benefit issues is designed to help users identify, measure and analyze relationships between monetary and nonmonetary costs and benefits in determining the program's return on investment. The second paper examines the pros and cons of different research strategies associated with the net impact approach. The final volume on MIS issues is to assist users in better understanding how JTPA and other employment and training *management information systems* can efficiently support the evaluation of program implementation and impact.

## THE SET OF VOLUMES

The set is *integrated*, but affords *flexible use*. The user can utilize the entire set for comprehensive evaluations over a two-year planning cycle or longer planning period, or the user can apply the information in each volume independently, based on the most pressing evaluation priorities and timeframes and given the extent of resources, during a particular fiscal year or biennium.

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It should be understood that although evaluation products have been developed for JTPA, their basic principles and methods can be applied more broadly by states and local areas to evaluate other employment and training programs and other social programs.

## GENERAL ACKNOWLEDGMENTS

The JTPA EVALUATION DESIGN PROJECT was developed and carried out based on the partnership philosophy that underlies the JTPA legislation. Several partnerships should be recognized for their substantial contributions to the products previewed here: the project development and coordination partnership; the public-private funding partnership; the interdisciplinary design partnership; and the advisory partnership.

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Terry R. Johnson

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## CHAPTER 1

# INTRODUCTION

# CHAPTER 1. INTRODUCTION

The Job Training Partnership Act (JTPA) of 1982, which officially replaced the Comprehensive Employment and Training Act (CETA) on October 1, 1983, continues CETA's stated objective of providing job training to improve the economic well-being of trainees. In particular, Section 106 of the Act explicitly states that "job training is an investment in human capital," and that the "return on the investment is to be measured by the increased employment and earnings of participants and the reduction in welfare dependency." Although the major objectives of the two programs are similar, the passage of JTPA significantly changed the employment and training system and resulted in states and local areas having much greater responsibility for program accountability under JTPA than in the past. This has, in turn, increased the need for reliable information on program effectiveness at the state and local level.

At the same time that the need for information on program effectiveness is increasing, however, the federal government is greatly reducing its role in providing such information. As a result, states do not have access to reliable research that could assist them in making informed and objective judgments concerning the effectiveness of JTPA programs and meet their increased oversight responsibilities under the Act. Moreover, states that might be interested in conducting their own analysis face the difficult problem of choosing a research design from among the myriad of approaches that have been used to examine the effectiveness of employment and training programs at the national level, and then tailoring it to meet their particular analysis needs and capabilities. Without a standardized approach to analyze the effectiveness of their programs, relatively little program analysis research has been conducted at the state level, and the studies that have been done have generally used inadequate approaches. To fill this gap, in this manual we describe a net impact evaluation model that most states could implement to provide much needed information on the effectiveness of their JTPA programs. Specifically, this manual serves as an implementation guide that describes in step-by-step terms how to estimate the net impact of JTPA on earnings and welfare dependency to help determine the return on the investment of job training programs funded under JTPA.

Before describing the net impact model, it is important to understand the significance of the term "net" and how a "net" impact model differs from a "gross" impact model. A gross impact analysis essentially compares the post-program labor market experiences of participants with their pre-program experiences and attributes all gains to the program. Although a carefully designed gross impact analysis can provide useful information on the relative effectiveness of different JTPA program activities, because numerous other factors may have changed from the pre-program to the post-program period that affect participants' labor market experiences (e.g., improvement in labor market conditions), such an analysis is not likely to isolate the true benefits of program participation per se, and should not be used to measure the return on the job training investment. On the other hand, a net impact analysis compares the labor market experiences of participants with the experiences of a comparison group of otherwise similar nonparticipants. The comparison group is used to approximate what the labor market experiences of participants would have been in the post-program period had they not participated in the program. As such, a net impact analysis only attributes to program participation the incremental gain in labor market experiences that occurs over and above what would have happened had these individuals not participated in the program. This is the appropriate concept for providing information on the return on the investment of job training programs funded under JTPA.

The state-level model described in this implementation guide is based on a detailed review of the employment and training program evaluation literature and a thorough examination of alternative approaches to conducting net impact analysis. The literature review, the issues involved in conducting a net impact analysis, and the advantages and disadvantages of alternative approaches are described in a companion evaluation guide (Volume V in this series). The evaluation guide is intended to serve as a reference document for those interested in the technical details and in the reasons why a particular option was chosen. This implementation guide provides very brief justifications for why a particular approach was chosen and instead focuses on how to implement the recommended approach and how to deal with some of the more important problems that are likely to arise.

In developing a state-level net impact model of JTPA, we were guided by several considerations. First, in order for the model to assist states in meeting their new accountability responsibilities, the model must be designed to produce valid estimates of the net impacts of JTPA programs on relevant participant postprogram outcomes. Second, the model must be usable and provide meaningful information that can be understood by a nontechnical audience in a cost-effective and timely manner. Finally, to maximize usability, the model must recognize the severe resource constraints that states and SDAs face, as well as certain other practical considerations. The two most important practical considerations that affected the recommended approach are (1) states and local SDAs will not generally be willing to implement an experimental design in which eligible applicants are randomly assigned to treatment-control status, and (2) states and local SDAs will not generally be willing to conduct follow-up interviews with a large sample of participants and comparison group members.

The state-level net impact model described in this implementation manual attempts to meet these objectives while recognizing the inherent tension between them. Because the proposed net impact model relies exclusively on administrative data from several agencies that are collected as part of the normal operating process in most states, we believe that the model is usable and that the analysis can be conducted in a timely manner and within the resource constraints that states and SDAs face. At the same time, however, such an approach necessarily limits the questions that can be addressed and also limits the variables that can be used to adjust for various analytical problems that are likely to arise and that could threaten the validity of the analysis. To the extent that the model is usable and provides valid results on important post-program outcomes (e.g., earnings, welfare dependency), then an important by-product, namely consistency in application across states, will occur. This will maximize the information obtained from the analyses and make an important contribution to what is known about the effectiveness of employment and training programs in states that face different environments.

The remainder of this implementation guide is organized as follows. In Chapter 2, we describe the key elements of the conceptual framework for a state-level JTPA net impact model. In particular, we indicate the outcome measures to be examined, the subgroups of trainees for which impacts should be separately measured, the program activities (treatments) that should be examined, the types of economic and demographic characteristics that may affect these outcomes, and the data sources that will be used to measure these elements.

In Chapter 3, we describe a research design for analyzing the net impact of JTPA. The research design includes a description of how to select a reliable comparison group and also offers guidance on how to select a representative sample of JTPA participants and sufficient numbers of participants and comparison group members to provide valid net impact results.

In Chapter 4, we discuss the steps involved in obtaining and processing the various data sources for the state-level net impact model. In particular, we describe the elements of a data collection and processing cycle, indicating potential problems that may arise in the process, describe how the various data sources should be organized, the types of data cleaning that should be performed, and briefly describe key features of the analysis files that should be constructed.

In Chapter 5, we present a data analysis plan to estimate the net impacts of JTPA programs on participants' post-program outcomes. This section begins with a description of methods for determining the quality of the comparison groups selected. We then describe a recommended approach to estimating the average net impacts of JTPA, and the net impacts for major subgroups, on the key participant post-program outcomes indicated in the legislation. We also discuss potential threats to the validity of the analysis and indicate possible approaches for adjusting for such problems.

In Chapter 6, we describe how to conduct a cost analysis to determine the costs of JTPA services. We also indicate how the cost information should be combined with the net impact estimates to provide information on the return on the investment of job training programs funded under JTPA.

Finally, in Chapter 7, we briefly indicate how the net impact analysis complements the other models being developed and how information from the other models may be useful in conducting the net impact analysis and in interpreting the net impact results.

# CHAPTER 2
# CONCEPTUAL FRAMEWORK

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**General Research Questions**
**Participant Outcomes**
**Participant Subgroups**
**Program Activities**
**Program Environmental Conditions**

# CHAPTER 2. CONCEPTUAL FRAMEWORK

An important element in the design of a state-level JTPA net impact model is the conceptual framework. The conceptual framework identifies the key research questions to be addressed, the outcomes to be examined, the participant groups and program activities (treatments) to be included, and the specific definitions of the outcomes, treatments, and variables that affect the relationship among treatments and outcomes. Because the net impact model is based exclusively on available administrative data, the conceptual framework is in large part data-determined.[1] In this chapter, we describe a conceptual framework for conducting a state-level JTPA net impact analysis based entirely on available state administrative data sources.

## GENERAL RESEARCH QUESTIONS

Although an employment and training program such as JTPA may affect different groups (e.g., participants, employers, government, or society as a whole) in different ways, the primary goal of the state-level JTPA net impact model is to determine the extent to which JTPA programs improve the labor market experiences of participants relative to what their experiences would have been in the absence of the program, that is, relative to a comparison group of otherwise similar nonparticipants. The average net impacts of JTPA programs on participants' post-program labor market experiences will provide policy-makers with an indication of the overall gains due to these programs that will more than meet state's increased accountability responsibilities under JTPA. Moreover, although it is important to know whether the mix of JTPA programs is effective on average, for policy purposes it is perhaps more important to probe beneath the average impacts to identify the relative effects on different subgroups. For example, to improve future targeting practices it is important to know whether certain participant groups benefit more from JTPA than other subgroups, and whether the net impact of JTPA differs among program activities (treatments) or length of participation. In addition, it is important to determine the extent to which net impacts vary by local environmental conditions (e.g., unemployment rate). Because previous national studies did not have the necessary data to address this latter issue, it may be possible for the state-level model to make a unique contribution to what is known about the effectiveness of employment and training programs that operate in different environmental conditions.

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[1] Even though the conceptual framework is constrained by the features of available state data bases, it is important to note that virtually all previous national studies of the net impacts of employment and training programs share several of the limitations of the state-level model. The net impact evaluation guide (Volume V) discusses this issue in detail.

The general objectives of the state-level net impact model can be summarized by a series of key research questions to be addressed:

- What is the overall net impact of JTPA programs on participants' post-program labor market experiences?
- How do the net impacts change over time?
- For which program activities (treatments) are the net impacts the largest? That is, which program activities result in the largest net benefits to participants?
- For which groups of participants are the net impacts the largest? That is, which subgroups gain the most from participating in JTPA?
- Do individuals who remain in JTPA longer experience greater net gains in labor market outcomes?
- How does the net impact of JTPA vary by local program environmental conditions?

These are the general research questions that the state-level JTPA net impact model will examine. In Exhibit 1 we survey some the key elements of the conceptual framework of the net impact model that help to make these questions more specific by indicating the outcomes that will be used to measure participants' labor market experiences, by indicating the participant subgroups to be examined, by identifying the types of program activities that will be examined, by identifying the local program environmental conditions of interest, and by indicating the various data sources that will be required. Below we discuss each of these key elements of the conceptual framework.

## PARTICIPANT OUTCOMES

As indicated in Exhibit 1, the state-level JTPA net impact model will focus on the following post-program participant outcome measures:

- Quarterly, semi-annual, and annual earnings and employment status based on UI Wage Records; and
- Quarterly, semi-annual, and annual AFDC grants and AFDC participation status based on Public Assistance (PA) Grants Records.

Because UI Wage Records are maintained on a calendar quarter basis, the earnings measures for longer periods can be created directly as sums of quarterly earnings values. The AFDC grants measures can be calculated as the sum of monthly grant payments. The employment status and the AFDC participation status outcome measures will be defined as

Exhibit 1

## KEY ELEMENTS OF THE CONCEPTUAL FRAMEWORK FOR THE JTPA NET IMPACT MODEL

### Outcome Measures

- Quarterly, semi-annual, and annual earnings and employment status based on UI Wage Records
- Quarterly, semi-annual, and annual AFDC grants and AFDC participation status based on PA records

### Participant Subgroups

- Adult men and women (to the extent possible, net impacts will be disaggregated by age, race/ethnicity, education, and for women, by welfare status)

### Program Activities

- Classroom Training (perhaps separately for remedial education and basic skills versus specific occupational skills training)
- On-the-Job Training
- Job Search Assistance
- Other

### Labor Market Conditions

- Unemployment Rate
- Urban/Rural Location

### Data Sources

- JTPA MIS (Participant characteristics, program activities, placement experiences)
- PA Grants Records (AFDC grants, whether received AFDC)
- UI Wage Records (Quarterly earnings, whether employed)
- UI Benefit History (Pre-program UI payments received, whether received UI)
- Local Labor Market Information (Unemployment rate)

categorical or dummy variables--e.g., 1 if employed during the period, 0 otherwise--in each of the different post-program periods of interest.

These outcomes are consistent with the major objectives of the legislation and, for the most part, greatly exceed the outcomes examined in previous national studies of the net impacts of employment and training programs. They also capture the range of short-term and relatively long-term impacts that could be observed within approximately a two-year program analysis cycle. Although potentially valuable information on the mechanisms through which employment and training programs increase earnings could be provided by examining other outcome measures such as employment intensity (e.g., hours worked per week, weeks worked per year), hourly wage rates, job retention, or the additional outcomes listed in the legislation for youths, such outcome measures require the collection of survey data from both participants and comparison group members. This data collection effort would greatly increase the resources required to implement the net impact model, and thus reduce its usability. However, states with additional resources, or particular interest in some of these other dimensions of labor market experiences, are encouraged to collect the necessary survey data and follow the research design and analysis plans described in subsequent sections to estimate the net impact of JTPA on these additional outcomes.

As discussed in the evaluation guide, the administrative data sources recommended for constructing the key participant outcome measures have several advantages over other potential data sources. For example, relative to SSN earnings records that have been used in most previous national studies, UI Wage Records are not "top-coded" at the SSN taxable maximum; they are available on a much more timely basis; and because they are available quarterly, one has more flexibility than is possible with SSN data that are available only on an annual basis. Moreover, although survey data allow for much more flexibility--including a greater range of outcome measures and personal characteristics--UI Wage Records are not subject to interviewer biases; nor to problems that arise from some respondents reporting net (after-tax) earnings and others reporting gross (before-tax) earnings; and they are not affected by response-rate problems that can plague survey data collection efforts. Monthly AFDC grants from administrative records also have numerous advantages relative to data obtained through surveys, including timeliness of data availability, and not being affected by respondent reporting errors, interviewer biases or response-rate problems. As a result of these advantages and the fact that they are quite inexpensive to obtain, the net impact model has been designed to rely on these administrative data systems.

Although these administrative data sources have several advantages, it is also important to recognize their limitations. First, and foremost, the 12 states that are not wage-reporting will generally not be able to use the net impact model to examine earnings impacts at this time.[2]

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[2] The 12 wage-request states are Hawaii, Massachusetts, Michigan, Minnesota, Nebraska, New Jersey, New York, Ohio, Rhode Island, Utah, Vermont, and Wisconsin. The Deficit Reduction Act of 1984, however, requires that all states effectively become wage-reporting by 1988.

The potential exceptions are states like New York that, although it is a wage-request state, comparable earnings records are maintained by the Department of Revenue and could be used for the analysis, provided the necessary interagency agreements could be made (e.g., data confidentiality).

Second, states that do not have automated AFDC grants records available at the state level will not be able to easily use the net impact model to examine JTPA impacts on reducing welfare dependency. Third, because the model relies heavily on the availability of historical earnings and grants records to control for potential differences between participants and comparison group members in the pre-program period (see Chapter 5), states must be able to directly access (or retrieve from archives) at least two to three years of UI wage records and AFDC grants records for a given person at any one time. Although most UI Wage Records systems are quite similar across wage-reporting states and generally contain at least seven quarters of data at any time, there is considerable variation in state and local welfare administration and record-keeping practices, as well as differences in the degree of data automation and retrieval capabilities, that will present obstacles to implementing this component of the net impact model in certain states. Fourth, UI Wage Records do not generally include employees of federal, state, or local governments, self-employed individuals, or employees in certain other occupations.[3]

Finally, because the system is state-based, it is impossible to distinguish individuals who work across the border in a different state from individuals who do not work in covered employment. Thus, unless interstate agreements can be worked out to access UI Wage Records, one should be very careful in estimating the net impact of JTPA on earnings for large SDAs located near state borders. However, as we indicate in Chapter 5, the deficiencies of UI Wage Records (i.e., coverage and border problems) should not in general result in biased estimates of net program impacts.

## PARTICIPANT SUBGROUPS

The next conceptual framework issue concerns the participant groups for which the outcome measures described above will be examined to determine the net impact of JTPA programs. As indicated in Exhibit 1, the state-level net impact model has been designed for adults only. This is in part because the outcome measures listed above (earnings, employment, and AFDC dependency) are not appropriate for youths (particularly in-school youths), and no existing data sets include information on more appropriate outcome measures (e.g., schooling attainment, employment competencies) for both participants and comparison group members. Moreover, extensive evidence presented in the evaluation guide indicated the difficulty researchers have had in developing a reliable matched comparison group for youths who have limited pre-program earnings histories. Because of these problems, the net impact model has been designed only for adults, and states interested in examining youth issues should consult the implementation guide for the gross impact model or consider other more costly approaches for conducting a net impact analysis.[4]

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[3] For example, in the State of Washington, UI Wage Records also do not include earnings for the following types of employees: certain corporate officers, church employees, individuals paid exclusively on commission, domestics who work for employers who pay less than $1,000 per quarter, railroad employees, employees of small agricultural firms, casual laborers, and certain barbers/cosmetologists. As a result of these coverage gaps, approximately 80 percent of all state wages are included in the UI Wage Records data base.

A second issue concerns whether separate net impact models need to be developed for specific adult subgroups. As described in the evaluation guide, most previous studies estimated separate net impact models for men and women; some of these studies also estimated separate models for youths and adults, and others estimated separate models by race or ethnicity group. We recommend that separate net impact models be developed for adult men and women because of extensive evidence indicating that the relationship between earnings and other demographic characteristics is very different for these two groups. However, we do not believe it is necessary to estimate entirely separate net impact models by race or for other participant subgroups.

Even though entirely separate models will not be estimated for other participant subgroups, as we describe in Chapter 5, it is possible to design the analysis to examine whether the net impact of JTPA differs among other adult male and female subgroups of interest. For example, based on the previous literature, it will be important to investigate whether the impact of JTPA varies by the following participant characteristics:

- Age (less than or equal to 35 as compared to over 35);
- Ethnicity (whites as compared to blacks and Hispanics);
- Educational level (at least a high school graduate as compared to nongraduates); and,
- Welfare status for adult women (welfare recipients as compared to nonrecipients).

To the extent sample sizes are adequate and states have the necessary staff/computer resources, as indicated in Exhibit 1 we recommend that net impact estimates be derived for these subgroups of adult men and women in order to replicate the research questions examined in previous studies and to provide valuable information on targeting issues.

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[4] For example, states that are very interested in developing net impact estimates for JTPA Title II-A youth programs might consider implementing an experimental design, or alternatively, conducting (relatively expensive) interviews of participants and comparison group members to collect the detailed pre-program and post-program employment and schooling data necessary for reliable analysis, and then follow the research design and analysis plans described later in this implementation guide. States interested in such approaches, however, should first consult employment and training researchers who are knowledgeable in experimental design issues and questionnaire development to avoid the pitfalls that have plagued previous studies.

## PROGRAM ACTIVITIES

Another important element of the conceptual framework is the determination of the key program activities (treatments) to be examined and the development of consistent definitions of the treatment variables. Section 204 of the Job Training Partnership Act authorizes the expenditure of Title II-A funds for over 20 types of employment and training program activities. Although the list of potential program activities is quite extensive and seems to allow a virtually unlimited array of potential services, the major program activities provided under JTPA include classroom training (CT), on-the-job training (OJT), and job search assistance (JSA). In fact, nearly 90% of adult FY 1984 Title II-A enrollees participated in one of these programs. Although work experience was used extensively under CETA, only 3% of adult participants in FY 1984 were assigned to work experience programs. Thus, although it is important to include in the analysis participants who are assigned to all types of activities in order to get an unbiased estimate of JTPA overall, as indicated in Exhibit 1 it is likely to be most useful to examine the separate effects of CT, OJT, and JSA.[5] A brief description of each of these program activities is provided below.

- Classroom training involves basic or remedial educational training, or occupational skills training to ensure that individuals acquire the ability and knowledge necessary to perform a specific job for which there is a demand. Such programs are usually provided in a classroom or institutional setting.

- On-the-job training emphasizes the development of occupational skills in an actual work setting, normally in the private sector. The programs are designed for participants who have been first hired by the employer, and the training occurs while the participant is engaged in productive work that provides knowledge or skills essential to the full and adequate performance of the job.

- Job search assistance includes any training activity that focuses on the development or enhancement of employment-seeking skills. This service is provided to participants who need practical experience in identifying and initiating contact and interviewing with prospective employers. It is usually conducted in a structured setting and can include approaches such as job-finding clubs or instructions for self-directed job search methods.

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[5] If, however, work experience or some other program activity is used extensively in a particular state so that the sample sizes are sufficient to support precise net impact estimates, it is possible to follow the other procedures outlined in the implementation guide to estimate the net impacts for this activity.

In creating specific treatment variables to represent these program activities, several potential complications arise. First, although the broad definitions of the major program activities are generally accepted, there is likely to be considerable variation across SDAs in the contents of specific program activities such as length of assignment, occupation of training, hours per day. This makes it particularly difficult to create meaningful variables that represent a homogeneous treatment. At the same time, however, it is necessary to do a considerable amount of aggregation of activities that are generally similar but perhaps far from identical, because it is simply not possible to reliably estimate the net effects of the virtually unlimited number of program activities. Second, not only are there differences in the degree of treatment within program activity, there are likely to be large differences across SDAs in the nature of programs provided such that work experience programs in a particular SDA may be more similar to OJT programs in another SDA. Finally, the way in which the actual training activities provided are recorded in the program MIS can result in additional complications. For example, due to the lack of uniform national reporting requirements, some SDAs record participation in a job search workshop as job search assistance, while others record it as classroom training because the sessions are conducted in a classroom setting. Such differences in the content and recording of program activities across SDAs emphasize the importance of conducting a process analysis concurrently with the net impact analysis in order to develop meaningful and consistent measures of program activities.

As indicated above, the ways in which the treatment variables are defined will in large part be determined by the structure and content of the MIS. In addition, they will depend on the specific research questions of interest, and the sample sizes of individuals who participate in the given program activity. For example, to ensure that the treatment variables are as homogeneous as possible, it may be desirable to separate classroom training activities that focus on remedial education and basic skills from classroom training activities that provide specific occupational skills training. At the same time, however, if the number of individuals participating in each of these programs is too small to produce statistically reliable net impact estimates for the separate activities, it may be necessary to collapse these two variables into one that represents classroom training programs in general.

Thus, although the specific definitions of the treatment variables will depend on several factors, the following variables should be created for participants for potential inclusion in some models to examine impacts by program activity and other characteristics of the treatment:

- Participant dummy variable (1 if JTPA participant; 0 otherwise);
- Classroom training dummy variable (1 if CT participant; 0 otherwise);

  - Remedial education and basic skills dummy variable (1 if CT program in remedial education and basic skills; 0 otherwise);
  - Specific occupational skills training dummy variable (1 if CT program in specific occupational skills; 0 otherwise);

- On-the-job training dummy variable (1 if OJT participant; 0 otherwise);

- Job search assistance dummy variable (includes all employment/placement-related activities) (1 if JSA participant; 0 otherwise);

- Other activity dummy variable (1 if not a CT, OJT, or JSA participant; 0 otherwise);

- Occupation of training dummy variables (1 if in specific one-digit DOT code; 0 otherwise);

- Length of program participation in weeks; and,

- Number of hours of training per day.

Such treatment variables would enable one to replicate all of the questions examined in previous national studies of employment and training programs, as well as several additional questions of interest.

## PROGRAM ENVIRONMENTAL CONDITIONS

The final element of the conceptual framework concerns the program environmental conditions to be included in the net impact model. By program environmental conditions, we are primarily referring to characteristics of the labor markets within which the program operates, although major SDA characteristics could also be considered. As indicated earlier, because of data limitations, previous national studies were unable to include any program environmental characteristics in their models. As such, little is known about how the net impact of employment and training programs vary by program environmental conditions. At the same time, however, because of the nature of local program environmental conditions (i.e., there is no within-SDA variation on these conditions), it is important to recognize that it will only be possible to obtain reasonably precise estimates of a few key conditions, and only in states that have a large number of SDAs, and that exhibit considerable variation in the conditions across SDAs.

As indicated in Exhibit 1, we believe that it is most important to control for differences in local unemployment rates and whether the individuals are located in an urban or rural area. These are the factors that the evaluation guide indicated are most likely to affect the employment and earnings experiences of adult men and women.

The unemployment rate can be obtained from the Local Area Unemployment Statistics (LAUS), published by the Bureau of Labor Statistics. This information is available monthly at the state and county level and for over 1,000 cities with population of at least 25,000. Aggregate measures of the unemployment rate corresponding to the quarterly, semi-annual, and annual outcome periods of interest can be calculated as weighted averages of the monthly values, where the weights are the ratios of the monthly values of the number of individuals in the labor force divided by the sum of these monthly values over the outcome period of interest.

In constructing the unemployment rate variable for an SDA, it is also important to recognize that the monthly values will not generally be available for the precise area of interest. Depending on the size of the SDA, the area it serves may be either larger or smaller than the county or the city for which the information is available. In cases where the SDA serves multiple counties, one should calculate the appropriate labor market variables by aggregating over the counties served by the SDA. For example, to calculate the unemployment rate one would simply sum the number of individuals unemployed in the various counties served by the SDA and divide by the total number of individuals in the labor force in those counties. In cases where the SDA serves only part of a given county, and where no value is available for a smaller geographical area (i.e., a city), one is generally constrained to use the county value.

It may also be possible to provide some information on how the net impact of JTPA varies by different SDA service delivery strategies. The service delivery strategies to be examined should be based on their policy importance to the particular state doing the analysis. Moreover, to ensure that the strategies of interest are distinct and quantifiable, and that there is sufficient variation among SDAs to support the analysis, it is important that a process analysis be conducted. Thus, if states with a large number of SDAs (roughly 30 or more) are interested in obtaining some information on how the net impact of JTPA varies by a key service delivery strategy, they should first ensure that there are significant differences in this strategy across SDAs. Provided it is possible to quantify these differences, one could then use the variable created to determine how the net impact of JTPA varies across SDAs that differ in this strategy using the approach described in Chapter 5. In states with relatively few SDAs, it is very unlikely that such an analysis would provide reasonably precise estimates of the differential effects of the strategy of interest.

# CHAPTER 3
# RESEARCH DESIGN

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**Participant Group**
Sample Frame
Sample Exclusions
Selecting the Participant Sample
**Comparison Group**
Comparison Group Sample Exclusions
**Selecting the Comparison Group Sample**
**Sample Sizes for the Participant and Comparison Samples**

# CHAPTER 3. RESEARCH DESIGN

To provide valid estimates of the net impacts of JTPA programs on the earnings and AFDC dependency of adult men and women, a research design must be developed that contains several elements. First, a sample of JTPA participants must be chosen so that the results can be generalized to the state level, and so that the necessary data can be collected efficiently, with minimum burden to state and SDA data processing staff. Second, a valid comparison group must be chosen so that the impact of JTPA can be distinguished from the impacts of other factors that also affect earnings and welfare dependency. Third, the sizes of the participant and comparison samples must be determined so that program impacts can be measured with precision. Fourth, the data must be obtained and processed, and analysis files must be developed. Finally, an overall estimation strategy must be developed, and analysis models must be specified that can provide valid estimates of the net impacts of JTPA programs on the post-program outcomes of participants. In this chapter, we describe the first three elements of the research design for conducting a state-level JTPA net impact analysis; data collection and processing issues are described in Chapter 4, and the overall estimation strategy and the specific net impact models to be estimated are described in Chapter 5.

An overview of the major components of the first three elements of the research design is provided in Exhibit 2. Below we discuss each of these elements in more detail.

## PARTICIPANT GROUP

An important component of the research design is the development of the participant group. The two major issues in selecting the participant group concern: (1) the individuals to be included in the sample frame and (2) the procedure to be used to select participants from the sample frame for inclusion in the analysis. A recommended approach to each of these issues is described below.

### Sample Frame

The choice of the sample frame from which the JTPA participant group will be drawn is an important determinant of the external validity of the net impact analysis, i.e., the degree to which the findings can be generalized. The sample frame should be representative of all JTPA participants so that the analysis results can be generalized to the state level, rather than just to particular subpopulations of participants or individual SDAs. Based on a detailed review of the advantages and disadvantages of alternative procedures described in the evaluation guide, the state-level net impact model has been designed to

Exhibit 2

## KEY ELEMENTS OF THE RESEARCH DESIGN FOR THE JTPA NET IMPACT MODEL

### Participant Group

- Comprised of samples of adult men and women who enroll in JTPA in each quarter of a given program year.
- Individuals will be excluded from the sample frame if they are not between 22 to 64 years of age. Individuals will subsequently be excluded from the analysis samples if they have missing data on key JTPA services received (e.g., program activity, length of participation).
- Quarterly samples of JTPA participants will be selected randomly from the groups of adult men and women enrollees that are included in the sample frame to ensure that the sample is representative of JTPA participants in the state.

### Comparison Group

- Comprised of samples of adult men and women who are new ES registrants in offices in the areas served by the SDAs in each quarter of a given program year.
- Individuals will be excluded from the comparison sample frame if they are not between 22 to 64 years of age; if they are not economically disadvantaged; or if they participate in JTPA or receive significant ES services.
- Quarterly samples of comparison group members will be selected from the sample frame of new ES registrants using a stratified random process to ensure that ES registrants and JTPA participants are similar on a few key characteristics (e.g., welfare recipiency, UI recipiency).

### Sample Size

- Because the marginal cost of increasing sample size is very small, states are encouraged to include in the analysis as many participants and comparison group members as their staffs and computer resources can handle.
- As a guideline, a total analysis sample of 12,000 cases--divided equally between adult men and women, and between participants and comparison group members (i.e., 3,000 each)--should be adequate to meet most state's analysis needs.

include in the participant sample frame all JTPA enrollees during a given time interval, as indicated in Exhibit 2. Specifically, the participant group will be comprised of samples of adult men and women who enroll in JTPA in each calendar quarter of a given program year.

A sample frame of all JTPA enrollees from each quarter in a given program year has numerous advantages. First, it yields a representative sample of JTPA participants in which neither short-term nor long-term participants are oversampled, and which is not sensitive to seasonal differences in the characteristics of participants or program activities. Thus, it should be possible to generalize the results to the state level. Second, because the time period for selecting each participant cohort within the program year is not too long (i.e., three months), it should be possible to select quarterly samples of comparison group members that closely match participants on the timing of the pre-program decline in earnings. As indicated in the evaluation guide, this is particularly important for ensuring valid net impact results. Third, using an enrollee-based sample maximizes the amount of pre-program earnings and AFDC data available for the model, which is particularly important for states that do not retain extensive historical data.

On the other hand, it must be recognized that an enrollee-based sample frame has some disadvantages. For example, in order to avoid excluding long-term stayers from the analysis, an enrollee sample frame causes a delay in analysis findings relative to a terminee-based sample. In addition, because a given group of enrollees may terminate in several different quarters, with such a design it is more difficult to estimate earnings impacts that correspond to specific time periods after program termination (e.g., three months following the quarter after termination). As described in the evaluation guide, however, alternative sample frames suffer from other problems that were considered to be more severe, which led us to the decision to use an enrollee-based sample.

Although a participant group comprised of adult men and women who enroll in JTPA in each of the four quarters of a given program year yields a representative sample that can provide valid results, it has implications for the timing of project results and the length of the post-program observation period within an approximate two-year program analysis cycle. Specifically, given the variation in length of stay, the delays involved in obtaining the outcome measures from agency records (approximately three months) and the time required to build the data files, conduct the analysis, and prepare written reports (approximately six months), with an enrollee-based sample of participants one can obtain net impact estimates for the period one year following the calendar quarter after termination for the longest stayers only for the first quarter cohort, and only a three-month net impact estimate can be obtained for all four quarterly cohorts in approximately a two to two and one-half year cycle. Of course, by obtaining additional post-program UI and AFDC records for sample members, one could estimate longer-term impacts, although the analysis period would have to be extended even further.

### Sample Exclusions

Once the general sample frame is chosen, one must then determine whether certain types of individuals should be excluded. Although any such exclusions necessarily reduce the representativeness of the participant sample, there may be valid reasons for excluding some participants. For example, it may be desirable to exclude cases because they lack data on critical items or because they are clearly inappropriate to include in the analysis. Below we offer our recommendations on participant sample exclusion issues for the net impact model.

As described in the evaluation guide, most previous studies have incorporated some restrictions on participant age. Although there is no universal agreement on the specific age range to use, very young participants (e.g., under age 16) have been excluded because earnings is not an appropriate outcome measure for those likely to return to school, and very old participants (e.g., those age 65 and older) have been excluded because participation in employment and training programs among individuals eligible for retirement is very rare, and it is unlikely that a proper comparison group could be identified. Because the state-level net impact model is designed to focus on adults only, the participant sample will be restricted to individuals of at least age 22. Because of the difficulty of obtaining a valid comparison group for older participants, we also recommend that any individuals age 65 and older be excluded from the participant sample frame.

Individuals should also be excluded from the sample frame if they have missing data on key variables. For the most part, we do not expect that there will be severe missing data problems with the agency records to be used, and most problems can be dealt with in straightforward ways in developing the analysis variables, as described in Chapter 4.[6] A more difficult problem arises when information is missing on the treatment provided by JTPA. For example, one cannot estimate the net impact by program activity or by length of stay for individuals with missing information on program activity or for those who have incomplete data on the start and end dates of their JTPA participation. Although we expect there will generally be very few problems concerning the omission of program start and end dates (in part because length of stay information is necessary for adjusting certain performance standards for JTPA Title II-A programs), because there are no reporting requirements concerning program activity, it is likely that some cases will contain missing (or unusable) program activity information. Moreover, because the quarterly enrollee samples will be selected on an ongoing basis, it will not be possible to know for sure how many cases must be excluded for missing data problems until after the JTPA MIS data are obtained and merged for the sample selected. As such, we recommend that a slightly expanded participant sample initially be selected, and that individuals who are subsequently

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[6] The limited amount of missing data is in part a reflection of the procedures used by many agencies to assign "default" values when data are missing. Such procedures lead to measurement error, which can also introduce analytical complications as discussed in Chapter 5.

As indicated in Exhibit 2, we recommend that the quarterly samples of JTPA participants be selected on a random basis from the groups of adult men and women enrollees included in the sample frame. Although adult men and women are served in approximately equal numbers by JTPA overall (i.e., 52.8% of all Title II-A adult participants were women in PY 84), because this varies considerably across SDAs, we believe it would be prudent to first stratify the participant sample by sex before the analysis samples are selected, or else there may be insufficient numbers of either men or women for analysis purposes.[8] Choosing a random sample from the sampling frames, separately by sex, also has the major advantage of providing representative samples of JTPA adult men and women participants, so that the results can be generalized to the state level. Moreover, estimates of the net impacts of JTPA and of the differential impacts by program activity can be obtained in a straightforward manner (i.e., no weighting is necessary). In addition, by selecting participants randomly, and not based on the particular program activity they are enrolled in, an analysis of the types of individuals who are assigned to various JTPA program activities (i.e., targeting) is also straightforward.

In addition to stratifying the sample by sex, states that want to focus their efforts on specific subgroups of adult men or women (e.g., female welfare recipients, male or female high school dropouts) may also want to consider stratifying the participant sample and oversampling the subgroups of interest. In general, stratification is desirable only when the research questions of interest relate to subgroups that occur rarely, or that occur so frequently that their nonoccurrence is rare. Depending on the specific research questions of interest, one could consider stratifying on the basis of participant characteristics or by program activity. For example, because of the wide variation across states and SDAs in the use of work experience programs, states that are interested in examining the net impact of work experience programs would likely need to stratify and oversample participants of such programs. Moreover, because job search assistance generally constitutes a less intensive treatment that is likely to have a smaller average net impact, a much larger sample of participants in JSA would be needed to precisely measure the lower expected effect. Thus, for states that are very interested in precisely measuring the marginal benefits from JSA participation, such participants would have to be oversampled. States that may be interested in stratifying the participant sample and oversampling certain groups should consult a statistician or sampling expert to better understand the advantages and disadvantages of such an approach and the specific steps to be followed in drawing the sample and conducting the analysis.

## COMPARISON GROUP

To estimate the net impact of JTPA on participants' post-program outcomes, a method is needed to gauge what would have happened to participants had they not participated in JTPA. A standard approach for determining the net impact of a program is to compare the experiences of persons influenced by the program (JTPA participants) with the experiences of persons who are not influenced by the program (the comparison group). The comparison group is used to estimate what the experiences of the participants would have been in the post-program period had they not participated in the program. To ensure that the differences between the experiences of the participant and comparison groups can be attributed to the program, the comparison group must have characteristics similar to JTPA participants, particularly in terms of the characteristics related to JTPA eligibility, and the data available must be comparably measured for the two groups.[9] In addition, it is important that comparison group members not have received significant employment and training program assistance, and that a method be developed to verify that individuals in the comparison sample in fact did not receive JTPA services. Following we describe the recommended comparison group for the net impact model.

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[8] For example, in some SDAs women comprised as little as 25% of adult JTPA terminees in PY 84, while in other SDAs women were over 80% of all adult terminees in Title II-A programs in PY 84.

As indicated in Exhibit 2, based on a detailed review of several alternative data bases, we recommend that the comparison group be comprised of new ES registrants in offices in the areas served by the SDAs.[10] ES registrants have several advantages as a comparison group. First, data are available on several individual characteristics of interest, including some of those related to JTPA eligibility, which should generally be comparably measured with JTPA MIS data and, as such, can be included as control variables.[11] Second, like JTPA participants, new ES registrants also presumably experienced a recent decline in earnings. Finally, also like JTPA participants, ES registrants are in the labor force at the time they apply for assistance. As discussed in the evaluation guide, it is important to ensure that participants and comparison group members are similar in their attachment to the labor force, or else net impact estimates can be severely biased. In order to ensure comparability on pre-program labor force attachment, the comparison group should be drawn from new ES registrants in the same calendar quarter that participants enroll in JTPA. Following this procedure, other registrants who applied for ES assistance more than three months earlier and, as such, experienced their decline in earnings even earlier and who may have already returned to work, will be excluded from the sample frame for the comparison group.

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[9] According to the JTPA legislation, to be eligible for Title II-A programs, adults must be 22 years of age or older and be economically disadvantaged. The legislation defines economically disadvantaged to mean an individual who: (1) is a member of a family that in the six months prior to application received a total income of less than the OMB poverty level or less than 70% of the lower living standard income level, whichever is greater, given the person's family size; (2) is a member of a family that receives federal, state, or local cash welfare payments; (3) is receiving food stamps; (4) is a foster child for whom state or local support payments are made; or (5) is a handicapped individual who is economically disadvantaged but whose family is not, as permitted by the Secretary of Labor. The Act requires that at least 90% of Title II-A participants be economically disadvantaged and allows up to 10% of the participants to be individuals who are not economically disadvantaged provided they have encountered barriers to employment (e.g., limited English language proficiency, school dropouts, ex-offenders). In addition, the Act requires that recipients of AFDC grants and school dropouts be equitably served in relation to their incidence in the eligible population. To the extent possible, the comparison group should only include individuals who meet the explicit eligibility criteria and who are similar to participants on characteristics emphasized in the legislation.

[10] As described in the evaluation guide, JTPA applicants who are eligible but who do not participate (commonly referred to as "no-shows") are also a potential source for drawing a comparison group. The primary advantage of using these individuals as a comparison group is that they did go through many of the selection processes that participants did. Thus, they were eligible, decided to apply for JTPA, were selected to participate, and were assigned to a program activity. In addition, a major potential advantage is that

Although ES registrants have several important advantages as a source

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because these individuals applied to JTPA, comparable baseline data were collected for them, which maximizes the amount of pre-program information that could be used to construct control variables for the net impact model. The major disadvantages are that (1) these groups introduce an additional selection bias in that they may be systematically different because they chose not to participate (perhaps because they found a job); (2) the sample sizes of such groups may be too small to support precise net impact estimates; and (3) data on no-shows are not usually included in the JTPA MIS. Thus, although this approach has certain advantages, it raises additional questions and cannot be implemented unless states and SDAs consistently include no-shows in the data base.

[11] Based on a comparison of ES and JTPA data collected in the State of Washington, it appears that the following individual characteristics are comparably measured and could be included in the net impact model: age, race (white, black, Hispanic, American Indian/Alaskan Native, Asian/Pacific Islander), education (whether received high school degree or equivalent), handicapped status (whether has physical or mental impairment that is a substantial handicap to employment), occupation (primary DOT code of previous job), veteran status (whether a veteran, whether a Vietnam-era veteran, whether recently separated, and whether a disabled veteran), Food Stamps recipient, WIN registrant, and economically disadvantaged status. In addition, pre-program measures of UI earnings, AFDC grants and whether a UI recipient will also be available and comparably measured for both participants and comparison group members. Although this list is not as complete as one would ideally like--measures of marital status, family size, dependent children, ex-offender status, limited English-speaking ability, and detailed data on pre-program employment experiences are not available--it must be recognized that most of these characteristics were unavailable to previous national studies of the impact of employment and training programs. As such, this is not a limitation that is specific to the state-level model.

for drawing a comparison group, a few disadvantages must also be recognized. First, and most important, because of recent reductions in federal reporting requirements related to the ES, states are no longer required to report the number of economically disadvantaged applicants who are registered and served by the ES. Because economically disadvantaged is the major criterion for JTPA eligibility, and given the importance of ensuring that the comparison group be similar to participants on all characteristics that affect eligibility, it is important that the economically disadvantaged status variable be available for the net impact model.[12] It appears, however, that many states have decided to continue collecting information on the economically disadvantaged status of ES registrants, in the event that reporting requirements change again. States that no longer collect this information will have to modify the comparison group sample selection procedures, as described in Chapter 4.

A second potential disadvantage to using ES registrants as a source for drawing comparison groups concerns limitations in procedures for retaining historical data. In the past, most states have kept copies of data tapes with individual ES records (including registrant characteristics and ES services received) for a period of three to five years. In some states, however, individual-level data are purged after approximately one year, and backup tapes that are archived are not very accessible. In such states, it will be difficult to draw the four quarterly samples retrospectively at one time, as comparison group members for the first quarter cohort would already have been purged. Thus, such states will have to draw the comparison samples on an ongoing basis (i.e., on a quarterly or semi-annual basis), alter their purging practices, or be sure to retain historical data for at least 18 months to two years.

A final potential complication in using ES registrants to develop a comparison group concerns the possibility that the ES registrant file may be dominated by UI claimants in some areas. For example, in states and local areas in which ES offices are co-located with UI offices, or in which the policy is to actively monitor the job search efforts of UI claimants, this could result in a large proportion of UI claimants in the ES registrant file. Because it is inappropriate to compare the outcomes of JTPA participants with a sample that is dominated by UI recipients, it may be necessary to undersample UI claimants in the ES registrant file in some states to obtain a comparison sample that has a proportion of claimants similar to the JTPA population. In adjusting for this potential problem, it is important to note that UI claimant status as recorded on the MIS systems for ES and JTPA may not represent the same concept. In particular, for JTPA participants, being a UI claimant means that the person filed a UI claim and was determined to be monetarily eligible, whereas for the ES, claimant status refers simply to the filing of a claim for benefits and does not imply monetary eligibility. Because of this difference, a typical JTPA "claimant" is much more likely to receive UI benefits than a typical ES "claimant." As a result, to ensure that UI recipiency is comparably measured for the two groups, we recommend that the UI Benefit History file be used to develop a measure of whether the person was a UI recipient as described in Chapter 4. A decision on the appropriate rate for sampling UI recipients from the ES registrant file would then be based on this measure.

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[12] It should be noted, however, that the decreased emphasis on the economically disadvantaged measure might also introduce additional measurement error into this variable. That is, not only did ES staff previously have no real incentive to accurately record the status of the applicant (i.e., since ES services do not depend on whether a person is economically disadvantaged), but they now have even less incentive to do so. As a result, it is likely that ES offices would tend to underreport serving such applicants. Thus, to the extent that only ES registrants who are recorded as economically disadvantaged are included in the sample, their status should be measured reasonably accurately, which will minimize analytical complications due to measurement error. In the evaluation guide we discuss the biases that can arise because of measurement error in the dependent or independent variables.

Despite these potential disadvantages, we believe that ES registrants are the best source among existing state data bases for drawing a comparison group to examine the net impacts of JTPA. As a result, we now turn to discuss additional details related to drawing a sample of ES registrants, including cases that should be excluded, and procedures for drawing the sample.

### Comparison Group Sample Exclusions

Prior to selecting the comparison group of ES registrants, certain cases should be excluded from the sample frame to maintain comparability with the participant sample. In addition, it may be desirable to exclude from the comparison group individuals who are clearly not eligible for JTPA and who are likely to have earnings potential that is considerably different from the earnings potential of JTPA participants. Below we discuss sample exclusion considerations concerning the comparison group of ES registrants.

To maintain comparability with the JTPA participant sample, the group of ES registrants should be restricted to individuals over 21 and under 65 years of age. Moreover, if it turns out that no one in the JTPA sample is over, say, age 55, then the ES registrant sample should be similarly restricted. In addition, it is important to exclude ES registrants from the sample who are JTPA participants during either the pre-program, inprogram, or post-program period. This problem, known as "comparison group contamination," results in comparing participants with other participants and yields net impact estimates that are biased toward zero. To minimize this problem, one should compare the social security numbers of current and recent JTPA participants with the social security numbers of ES registrants and exclude all matches from the comparison sample.

A related exclusion issue arises in attempting to ensure that the comparison group is as untreated as possible. In particular, in addition to excluding from the sample frame potential comparison group members who have participated in or are participating in JTPA, it is also important to exclude individuals who receive significant ES services.[13] The major ES services include referral to jobs listed with the ES, job development, job counseling, and testing. Because individuals who receive such services may be receiving considerable employment assistance, we recommend that such individuals be excluded from the comparison group sample frame. Because a substantial proportion of ES registrants generally receive no services other than registration, there are likely to be a sufficient number of ES registrants who receive no services and who are similar to JTPA participants on measured characteristics to develop a comparison group. It should be noted, however, that because individuals often receive some ES services a few months after they apply for assistance, a waiting period after application will be required before the comparison sample frame can be reliably purged of individuals who receive potentially significant ES services. The appropriate procedures to be followed are described in Chapter 4.

A final important sample exclusion issue concerns procedures to ensure the similarity of the participants and comparison group members on characteristics related to eligibility for JTPA. As indicated above, the primary criterion for JTPA eligibility is that the person be economically disadvantaged. Although the Act requires that at least 90% of JTPA participants be economically disadvantaged, in PY 84, 95% of adults in Title II-A programs were economically disadvantaged. Moreover, of those who are not economically disadvantaged, or who cannot be certified to be economically disadvantaged, the Act requires that they face demonstrated employment barriers. Because virtually all adult Title II-A enrollees are economically disadvantaged, and even those who are not certified to be economically disadvantaged may in fact be so, or may have earnings potential that is most similar to economically disadvantaged individuals, we recommend that the comparison group also exclude all new ES registrants who are not economically disadvantaged at application. This will ensure a comparison group that is similar to JTPA participants on the key characteristic related to JTPA eligibility.[14]

## SELECTING THE COMPARISON GROUP SAMPLE

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[13] If ES registrants receive significant employment assistance, a comparison of the average post-program outcomes of JTPA participants and ES registrants would measure the incremental effect of JTPA relative to the ES, and not the effect of JTPA per se.

[14] As described in the evaluation guide, several previous studies excluded from the comparison group individuals with very high pre-program earnings who were clearly ineligible to participate in employment and training programs. By matching participants and comparison group members on economically disadvantaged status, however, such additional exclusions are no longer necessary. It should be noted that if the economically disadvantaged status variable is not available in some states for ES registrants, then procedures to exclude cases with high pre-program earnings would have to be implemented as described in Chapter 4.

Once decisions have been made on which individuals should be excluded from the comparison sample frame, the next step involves the procedures to be used for drawing the comparison group. Although several alternative procedures for selecting the comparison group were considered, as indicated in Exhibit 2 it was decided to select a stratified random sample of ES registrants that has the same distribution as JTPA participants on a few key characteristics. This approach maintains maximum statistical power to the extent possible, while ensuring that the participant and comparison samples are similar on key characteristics.

As indicated above, because of program eligibility considerations and certain practical issues concerning the relationship between the ES and other programs (e.g., UI, welfare), some of the more important characteristics on which to ensure comparability between participants and comparison group members are economically disadvantaged status, receipt of UI, and receipt of AFDC. Because we will ensure comparability between the two groups on economically disadvantaged status by excluding from the sample frame for the comparison group all new ES registrants who are not disadvantaged, no additional matching is required on this characteristic. Although we do not have any information on the proportion of new ES registrants that are AFDC recipients or that are UI recipients nationwide, there are likely to be relatively too few ES registrants who are AFDC recipients (e.g., 9% of adult men and 35% of adult women JTPA terminees in PY 84 were receiving AFDC at application) and relatively too many ES registrants who are receiving UI benefits (e.g., 15% of adult men and 8% of adult women JTPA terminees in PY 84 were UI claimants at application). In order to ensure similarity on these important characteristics, we recommend that comparison group members be randomly selected from the sample frames of adult men and women to match the distribution of participants on these characteristics. Thus, for the separate samples of adult men and women, procedures would be used to ensure that the participant and comparison groups are similar on the proportions in the four cells representing combinations of AFDC and UI recipiency status. Operationally, for a given total sample size of participants and comparison group members, sampling rates for each cell would be determined to match the two distributions, and then comparison group members would be selected randomly from the cells at the given sampling rates. The definitions of AFDC and UI recipiency status to be used and the procedures to match the two samples are described more fully in Chapter 4.

## SAMPLE SIZES FOR THE PARTICIPANT AND COMPARISON SAMPLES

An important element of the research design is the determination of the appropriate sample sizes for the participant and comparison groups. As we indicated earlier, in most states there will be little choice involving the size of the participant sample. That is, all JTPA participants may be necessary to derive reasonably precise estimates of average program net impacts. Because the marginal cost of increasing sample size is very low (i.e., data are available in existing agency files), even in medium to large states one should generally use the largest numbers of participants and comparison group members feasible, given available staff and computer resources. In states with very large JTPA programs, however, samples of participants and comparison group members will be drawn, which raises the issue of total sample size as well as the allocation of the total sample among the two groups. The evaluation guide contains a description of an approach that states can use to select appropriate sample sizes for the net impact model. Below we briefly summarize the results of that discussion.

The appropriate sample size for the net impact model ultimately depends on the size of the impact that is important to detect for policy purposes and the level of statistical accuracy required. With larger sample sizes, one has greater assurance of detecting small differences in overall outcomes between the participant and comparison groups, as well as detecting differences for major participant subgroups or across program activities. The likelihood of detecting a given difference in outcomes also depends on the allocation of the total sample between the participant and comparison groups and the unexplained variance of the outcome measure (i.e., earnings, AFDC grants). In general, the more homogeneous the samples, that is, the smaller the variance of the outcome measure, the smaller will be the number of cases necessary to detect a given difference in outcomes at a specific level of significance.

Based on a number of considerations described in the evaluation guide, we indicate that a total analysis sample of 12,000- divided equally between adult men and women, participants and comparison group members (i.e., 3,000 each)- should be adequate to meet most state's analysis needs.[15] It should be emphasized that this sample size recommendation refers to the final analysis samples and, because some cases will be omitted for various problems described above, initial samples will be somewhat larger as described in Chapter 4. States that are interested in obtaining precise net impact estimates for subgroups of adult men or women, or that have additional resources should consider larger sample sizes as necessary. Finally, states with relatively small JTPA programs (i.e., less than 2,000 adult enrollees per year) should be very careful in interpreting the results as only very large impacts will be regarded as significantly different from zero. As a result, such states might consider pooling samples with other states or over time to increase sample size to enhance the reliability of the net impact findings.

Although we believe that a total analysis sample size of 12,000 should generally be adequate to meet most state's analysis needs, it must be emphasized that the appropriate sample size depends on several factors and that there is no size that will be correct under all circumstances. As such, states that are unsure as to the appropriate sample sizes to use in a net impact analysis should review the evaluation guide and discuss their concerns with a sampling expert before making a final decision.

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[15] As described in the evaluation guide, with such a sample design we estimate that it will be possible to detect approximately a five (six) percentage point impact on earnings for adult men (adult women) with 90% power at a .10 significance level. That is, one would have 90% power with a .10 significance level of detecting an overall net increase in participants earnings of as small as five or six percentage points. The statistical power is the probability of detecting an effect at the chosen significance level when the effect of the specified size in fact exists (i.e., it is 1 minus the probability of making a Type II error).

# CHAPTER 4

# DATA COLLECTION AND PROCESSING PLAN

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Selecting the Participant Groups
Obtaining Preprogram Data for the Participant Groups
Selecting the Comparison Groups
Obtaining Preprogram Data for the Comparison Groups
Preparing a Preprogram Analysis File
Obtaining and Processing During Program and Postprogram Outcome Data
Obtaining and Processing JTPA MIS Data
Creating Net Impact Analysis Files

## CHAPTER 4.

## DATA COLLECTION AND PROCESSING PLAN

In order to implement the general research design described above, numerous data collection and processing tasks must be conducted. For example, quarterly samples of participants and comparison group members must be drawn, pre-program, during-program, and post-program data must be obtained from several sources (e.g., JTPA, ES, UI, PA) and merged to individual records, the data obtained must be cleaned, certain cases may need to be excluded, analytic variables must be created, procedures must be implemented to deal with missing data problems, and analysis files must be created. In this chapter we describe in detail the many steps involved in a data collection and processing cycle for the net impact model and discuss potential problems that may arise.

In the discussion below, we focus on the various data collection and processing tasks that must be conducted for a typical program quarter. In particular, we have chosen for illustration purposes the first quarter of PY 1985 (i.e., July 1, 1985 through September 30, 1985). Once the quarterly data files have been created, it is straightforward to merge these files to create a program year data file that meets the needs of the net impact model.

Although none of the data collection or processing tasks described in this chapter is particularly difficult or burdensome, it is important to recognize that the overall magnitude of these tasks is considerable. Moreover, many of the tasks ultimately must be performed by staff of several different agencies or subagencies. The size and breadth of the data related tasks have two important implications. First, in order to develop an effective data base system for the net impact model, there must be active participation, cooperation, and support on the part of several state agencies and subagencies on an ongoing basis. Because these agencies have different priorities and their own policies concerning issues such as data confidentiality, it is important that any issues of concern be resolved at the outset, that a regular data collection and processing schedule be established, and that the net impact model be given strong administrative support. Otherwise, the analysis may not be able to provide meaningful results in a timely fashion that will be of use to policy makers. It must be emphasized that the lack of support on the part of any of the agencies involved will considerably reduce the value of the net impact results and could potentially render them useless. Second, in order to ensure that the various data processing tasks are effectively coordinated, one person must be given the responsibility to manage these tasks and the authority to obtain the necessary staff and computer assistance. Managing this effort is likely to be a time-consuming activity in the initial stages, but once the system is in place and the individual tasks become routine, the management time required should decline considerably. Although strong administrative support and the appointment of a project manager are both necessary conditions for the effective management of virtually all projects, the magnitude of the data processing tasks and the involvement of multiple agencies make these conditions particularly important for managing the data processing system for the net impact model.

In Exhibit 3 we provide a general overview of the various data collection and processing tasks involved from sample selection to preparing an analysis file for estimating the net impacts of JTPA. In the remainder of this section, we describe the steps involved in conducting each of these tasks. In describing the steps involved in each major data processing task, we have attempted to be as specific as possible, and to carefully indicate the sequential nature of the steps. To ensure clarity, we have tended to err on the side of presenting as separate steps some activities that could easily be combined into one step. It should also be noted that in attempting to be very specific, we run the risk of being incorrect. That is, it is likely that a number of problems will arise in conducting a state-level analysis that have not been fully anticipated in this implementation guide. Without direct experience in implementing the net impact model using actual data on JTPA participants and ES registrants, and experience in merging UI Wage Records and AFDC Grants Records, it is not possible to fully anticipate the different kinds of problems that are likely to arise. Moreover, several of the recommendations provided below that rely on estimates of the occurrence of certain events (e.g., the proportion of JTPA participants or ES registrants that have certain characteristics) have been made without the benefit of detailed data on these issues. As such, although the recommendations/guidelines provided below constitute our best judgments at this time, some of these guidelines are based on little more than "best guesses." In order to improve the guidance available to subsequent users we would greatly appreciate being informed of unanticipated problems that occur, situations that are not adequately addressed in the implementation guide, or experiences that differ considerably from the general description provided here.

## SELECTING THE PARTICIPANT GROUPS

As indicated in Exhibit 3, the first data collection and processing task involves the selection of the participant groups. As described in the research design above, samples of adult men and women JTPA Title II-A enrollees will be selected on a quarterly basis. For illustration purposes, below we list steps that could be followed by JTPA data processing staff to select appropriate participant groups for the first quarter of PY 1985 (i.e., adult men and women who enrolled between July 1, 1985 and September 30, 1985):

1. Create a working file (on tape) of all persons who enrolled in JTPA Title II-A programs in any SDA in the state during the quarter (i.e., the first quarter of PY 1985). The file should include the person's SSN, age, and sex. Although the specific date of enrollment is not necessary for selecting the sample, it may be advisable to include it on the working file so that one

## Exhibit 3

## OVERVIEW OF DATA COLLECTION AND PROCESSING TASKS

- Select quarterly samples of participants and obtain, merge, and process pre-program data from various sources for these participants.

- Select expanded quarterly samples of new ES registrants who are economically disadvantaged to serve as potential comparison group members. Obtain, merge, and process pre-program data from various sources for the expanded samples. Select quarterly comparison groups of adult men and women ES registrants who received no ES services from the expanded samples to match the distribution of participants on four cells comprising combinations of UI recipiency status and AFDC recipiency status.

- Create quarterly pre-program data files that include all of the data obtained in the above steps for the samples of participants and comparison group members.

- Create separate annual pre-program analysis files for adult men and women from the quarterly pre-program data files. This involves merging the quarterly files, editing the data, creating analytic variables, and implementing procedures to handle cases with missing data.

- Obtain in-program and post-program outcome data (i.e., AFDC Grants Records and UI Wage Records) for all quarterly samples of participants and comparison group members and create appropriate outcome variables.

- Obtain data on JTPA services for participants in each of the quarterly samples and create treatment variables.

- Create separate annual net impact analysis files for adult men and women by merging the pre-program analysis files with the outcome and treatment variables.

could check to make sure that the extraction program worked properly (i.e., that all persons on the file enrolled in JTPA during the particular quarter of interest and that enrollees during a particular part of the quarter were not inadvertently omitted).

2. Exclude from the file all persons who are age 21 or younger, or age 65 or older.

3. Stratify the participant group by sex. That is, create a subfile of adult men aged 22 to 64 and a separate subfile of adult women aged 22 to 64.

4. Select a random sample of adult men and women participants from the two separate subfiles and determine the maximum age for men and women in the selected samples. (Information on the age range will be used in selecting comparison group members as described below.) The samples should be selected using the last two digits of the SSN, which are random numbers. The size of the quarterly samples should be such that the final analysis samples for the program year will include at least 3,000 adult men and women each. In order to allow for a small--5% or so--expected sample loss due to potential missing data problems (i.e., JTPA cases will be subsequently excluded if they have missing data on key variables such as length of stay or program activity), the initial sample of adult men and women participants for the entire program year should be approximately 3,200 each. To account for potential seasonal differences in JTPA enrollments (perhaps resulting from offering programs that only begin at certain times), one should select a fixed proportion of enrollees in each of the four quarters, and thus let the size of the participant group selected vary across quarters to reflect seasonal differences in enrollments. Because the quarterly samples must be selected long before the total adult enrollment is known for the year, however, it is not possible to set a fixed SSN range to generate an initial adult participant sample for the program year of precisely 3,200 men and women each. The simplest approach, and one that should work reasonably well provided total enrollments are similar from year to year, or that enrollment changes can be reasonably well anticipated, would be to calculate the appropriate SSN range based on total adult enrollments in the previous year. That is, if the state sample design calls for final analysis samples of 3,000 adult men and 3,000 adult women over the year (or approximately 3,200 initial enrollees to account for the 5% expected sample loss due to missing data), and if 8,000 adult men and 6,400 adult women enrolled in JTPA Title II-A programs in the previous year throughout the state, then an SSN range of 00 to 39 (i.e., a 40% sample) for men and 00 to 49 (i.e., a 50% sample) for women applied uniformly in each quarter of the program year should result in approximately the expected total sample size for the entire program year, although the number of

participants selected would vary across quarters.[16]

Following these four steps, one could obtain a representative sample of adult men and women JTPA enrollees in the first quarter of PY 1985. By repeating these steps for the subsequent three quarters (and keeping the SSN range fixed, unless the resulting quarterly sample sizes are insufficient), one could generate representative samples of approximately 3,200 adult men and 3,200 adult women who enrolled in JTPA Title II-A programs in PY 1985, which should be sufficient to generate final analysis samples of 3,000 adult men and women each.

It should be noted that in using enrollments from the prior program year to set the SSN range in Step 4 above, it may be necessary to adjust estimated enrollments to reflect changes in real program resources (i.e., changes in federal allocations adjusted for inflation). If real resources increase (decrease), then we should expect enrollments to increase (decrease), but not proportionately. In order to estimate the expected change in enrollments more precisely, one needs information on the percentage change in program costs that are typically incurred for a given percentage change in the number of JTPA participants. That is, one needs to know what the elasticity of program costs is with respect to the number of participants. One can derive an estimate of this elasticity from the cost analysis described in Chapter 6. Given an estimate of the elasticity and of the percentage change in real resources, one can develop an estimate of the likely change in JTPA participants that can be used in setting the SSN range.

Adjustments may also need to be made for changes in the types of programs offered that may affect the number of participants served. In making such adjustments one should carefully review SDA plans. It is also important to monitor the overall quarterly enrollments and the sample sizes obtained each quarter and compare them with the quarterly enrollments during the previous year and the expected program year samples necessary for analysis. If it appears that the SSN range being used is not adequate to generate the necessary samples, then one should expand the range accordingly.

Although these steps could be used by all states, some of the steps may not be necessary in some states and other states may need to alter the procedures slightly to meet their needs. For example, some states may not need to stratify by sex before setting the SSN range to select the sample. That is, states that serve adult men and women in JTPA in approximately equal proportions could omit step (3), and set the SSN range to get a total initial sample of 6,400 adults for the entire program year. In the illustration used above, to get a random sample of 6,400 adults from a total of 14,400 enrollees, a 44% sample is required, so that an SSN range of 00 to 43 could be used. If this procedure were used, we would expect to end up with approximately 3,560 men and 2,840 women (i.e., a 56/44% split), which is not too different from the recommended initial sample of 3,200 each. Because the statistical precision of the net impact analysis is not very sensitive to such relatively minor changes, given the large sample sizes involved, such a state could omit the step involving sample stratification by sex if it so chooses. Ignoring the prestratification by sex is particularly acceptable for states that serve relatively more men because, as we indicated in the evaluation guide, for a given sample size for women, somewhat larger samples of men are required to achieve the same statistical power. Thus, as a general guide, if the population of adult JTPA participants in a state is comprised of less than 45% men or more than 60% men, the participant group should be stratified by sex before the sample is selected. If the proportion of men served is between this range, such prestratification should be considered optional. However, because minimal effort is required to stratify the samples and no additional analytic complications are introduced (since the models are to be estimated separately by sex), prestratification by sex is recommended.

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[16] Because the last two digits of the SSN are random numbers, it should be noted that any range or preselected set of digits will work equally well in generating representative samples. That is, a 50% sample could be generated using a range of 00 to 49, 50 to 99, 25 to 74, or all odd or even numbers.

Minor modifications to the above steps may also be made to accommodate states that desire larger samples, either to obtain greater statistical precision in general, or to examine net impacts for specific subgroups of interest. To obtain a larger random sample of adult men and women JTPA enrollees, one can simply increase the acceptable SSN range as necessary within the limits of total available enrollees. For example, to double the sample from 3,200 to 6,400 men and women enrollees each per year, using the illustration provided above one would include all women enrollees and select an 80% sample of adult men (e.g., include all adult men whose SSN ended in 00 to 79 or some equivalent range). If a state is interested in oversampling certain subgroups of adult men and women to improve its chances of detecting whether these subgroups benefit to a different extent from JTPA participation, it should consult a statistician/sampling expert for advice on how to select the sample, as well as how to correct for the differential sampling probabilities when examining the net impacts of JTPA.

## OBTAINING PRE-PROGRAM DATA FOR THE PARTICIPANT GROUPS

Once the samples of adult men and women JTPA Title II-A enrollees have been selected for a specific program quarter, the next step involves obtaining several different data elements for these individuals and merging the data onto the person's record. In particular, as we indicated in Chapters 2 and 3, it is important to obtain each participant's UI Wage Records, AFDC Grants Payment Records, and UI Benefit History for the pre-program period. Moreover, it is very important that this information be obtained soon after the participant group is selected. The necessity for timely acquisition of these data is because (1) some states do not retain much historical UI earnings or AFDC grants data, and thus the sooner the data are obtained, the longer will be the pre-program period covered; and because (2) some of these pre-program data items will be used to develop individual characteristics that will in turn be used to select comparison group members that are similar on these characteristics. Below we describe the various data elements that are to be obtained and merged to the quarterly samples of adult JTPA enrollees selected and indicate some of the problems that may be encountered in this process.

The first set of data elements to be obtained for the quarterly samples of JTPA enrollees is their JTPA application information. Because the list of SSNs used to select the quarterly participant samples was originally obtained from the JTPA MIS, it should be straightforward to, in turn, obtain the program application information for these individuals. For various analysis purposes, it will be useful to initially extract all application data from the JTPA MIS system for those individuals selected into the quarterly participant samples. Although only a subset of the data items will be used for the net impact analysis (i.e., only those that can be regarded as comparably measured for participants and comparison group members), all application data should be obtained in case states are interested in using the net impact design to estimate gross impact models, or to examine whether assignment to program activity depends on other characteristics that are available for participants, but not for comparison group members. After extracting the data, JTPA data processing staff will provide the net impact staff with a file that includes the SSN and the entire set of JTPA application data for each person. The net impact staff will subsequently develop measures of individual characteristics from this file for inclusion in the net impact model.

In addition to obtaining JTPA application data, as emphasized in the general research design discussion above, detailed pre-program earnings and AFDC grants histories are very important to the net impact model. Ideally, one would want at least three years of pre-program earnings and AFDC data, although differences in recording and data retention practices may make this very difficult to achieve in some states. To obtain pre-program UI Wage Records and AFDC Grants Records for the quarterly samples of JTPA participants, it is necessary to provide the appropriate agency staff with a computer tape that includes the SSNs of all adult JTPA participants (men and women) selected, along with a memorandum containing a clear and concise description of the information requested. Whether the tape should be ordered in ascending SSN or some other method depends on how the UI and PA data bases are organized in the particular state.

There are generally six to twelve quarters of UI wages available at any one time, with approximately a three-month lag in the processing of these data. Thus, for example, if in October 1985, one were to request the quarterly UI wage records of individuals who enrolled in JTPA during the first quarter of PY 1985 (i.e., between July 1, 1985 and September 30, 1985), one would almost always be able to obtain records containing quarterly wages earned since January 1984, and in some cases beginning as far back as July 1982. The most recent wage information that will have been processed by October 1985 will be for the period from April 1, 1985 through June 30, 1985, the immediate pre-program quarter. Based on discussions with UI data processing staff, it appears that although wages for prior quarters will almost always be complete by this time, wages for the immediate pre-program period will be updated for some individuals after October, and will not be complete until around January 1986. As a result, in order to ensure that the wages for the immediate pre-program quarter are complete for the analysis, it will be necessary to collect data for this quarter again when the post-program earnings data are obtained.

At approximately the same time, a request will also be made for information on the pre-program monthly AFDC grants received by these JTPA participants. In addition to obtaining pre-program monthly grants records for up to three years (or even longer if readily available), in order to define welfare recipiency status similarly for JTPA participants and ES registrants, we recommend that data on AFDC grants received for the month after the month of enrollment also be obtained for all individuals at this time. Thus, as part of the initial request, AFDC grants data for the month after the end of the quarter of enrollment of interest (i.e., for October 1985) will also be obtained. Because there are generally minimal lags or delays in acquiring these data (i.e., a few weeks), this indicates that by the middle of November one should be able to obtain monthly AFDC grants data from the pre-program period through October 1985. These monthly grants records will be used to create quarterly measures of welfare grants received that will serve as important control variables in the estimation models described in Chapter 5. In addition, these monthly grants records will be used to create an "AFDC recipient" characteristic that is comparably defined for participants and comparison group members. To minimize problems caused by differences in the length of time from JTPA application to enrollment, or from differences in recording practices across SDAs, we recommend that a JTPA enrollee be defined as an AFDC recipient if she or he received AFDC grants during the calendar month prior to JTPA enrollment, the month of enrollment, or during the month after enrollment. A similar definition would be used to define AFDC recipient status for comparison group members, except that the three different months would be defined relative to the month of application to the ES. Although this definition may differ from the definition used in JTPA to define welfare recipient status, it will enable one to draw a comparison group that is similar on this important characteristic.

Although it should be quite straightforward to collect the UI Wage Records for the quarterly samples of JTPA participants, the PA data systems in some states may present obstacles to obtaining accurate pre-program AFDC Grants Records for certain types of individuals. To ensure that the AFDC records accurately represent the welfare recipiency status of these individuals in prior months to the extent possible, it will be necessary for the manager of the net impact model to meet with the AFDC data processing experts to discuss situations that could lead to obtaining inappropriate data for some AFDC recipients. For example, based on discussions with PA data processing experts in Washington State, we have identified two situations that present obstacles to obtaining accurate AFDC grants paid in previous months, and these or similar problems could occur in other states as well. These two potential problems are:

1. Although it is possible to identify the monthly grants paid to a particular assistance unit for up to three prior years that a person with a particular SSN is currently in (using the

"Recipient History File" that has an SSN to current assistance unit conversion file), it is possible that the specific person was not in that particular assistance unit throughout the three-year period. This is because the history file only has the amount paid to the unit in previous months, and the SSNs of individuals who are currently in the unit and contains no information on who was in the unit in previous months. As a result, the pre-program AFDC history for the unit may not accurately reflect a person's welfare recipiency status during this period. This is particularly a problem for individuals who experience a marriage or divorce, or who change living arrangements.

2. Because the data file is an index-sequential file, it requires that the recipient can only be on the file once (i.e., it requires that SSNs be unique). This can cause problems because there are two types of cases on the Washington State public assistance recipient file, namely AFDC and Food Stamps cases. For example, if a person who receives AFDC is coded as a Food Stamps case during a given month (perhaps because someone moved into the household who had income that was too high for AFDC but low enough to be eligible for Food Stamps), then the entire 3-year AFDC history is not available for that person. Thus, even though the welfare system has two folders of information on this particular person, the data base as designed is only capable of showing the person as either an AFDC case or a Food Stamps case, and if a person on AFDC is shown as a Food Stamps case, the data base would not contain information on the pre-program AFDC grants received by this person.

In order to obtain accurate information on the pre-program AFDC status of participants (and comparison group members), it is necessary to overcome problems such as these to the extent possible. To deal with the first problem listed above, the only potential solution available in Washington State appears to involve using the "Warrant Roll Extract Files," which contain a record of all AFDC payments made each month and a list of all SSNs in the household that month. Although these monthly data files are currently available for over 10 years (in data archives), it would be very cumbersome to use these files to track payments to specific individuals through time. It is likely that the relatively small improvements in the pre-program AFDC data that would be achieved through such a process would not be worth the high costs of processing these numerous data files.[17]

To deal with the second problem in the State of Washington, a three step process is required, as follows:

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[17] This is particularly true for correcting minor errors in pre-program data. That is, as discussed in the evaluation guide, measurement error is a much more serious problem in the dependent variable (i.e., post-program measures of AFDC grants) than it is in independent variables (i.e., pre-program measures).

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1. Identify all SSNs in the participant group that are listed as a Food Stamps case (i.e., that have an "F" case record).

2. For each SSN identified in Step 1, determine whether this person also has an AFDC record (i.e., also has a "C" case record). This step involves a manual review of the person's file folder.

3. Obtain the monthly AFDC grants data for the subset of "C" cases identified in Step 2, and merge them to the data file that contains historical AFDC grants data through October 1985.

These three steps will enable one to overcome this limitation of the PA data system in the State of Washington and obtain accurate AFDC historical data for persons who enrolled in JTPA in the first quarter of PY 1985. After completing these additional steps, PA data processing staff will return a data tape to the net impact project staff that includes the SSN, the month of enrollment, and the monthly AFDC grants received from July 1982 (and even before if available) through October 1985. Individuals who do not match SSNs on the PA data base (i.e., those who received no AFDC grants during the three-year period) will have each of the monthly values set to zero. Similar procedures would be followed to obtain historical data for subsequent samples of quarterly enrollees and for all comparison group samples.

In addition to obtaining historical UI Wage Records and AFDC Grants Records, it is also necessary to obtain historical information on the UI benefits received by participants. As indicated earlier, this information will primarily be used to create a comparable definition of UI recipiency status for participants and comparison group members that will in turn be used in selecting comparison group members to ensure that the comparison group does not contain an excessive number of UI recipients. In order to develop a consistent measure of UI recipiency status for both groups, as discussed in the evaluation guide we recommend that a JTPA enrollee be defined as a UI recipient if she or he received UI benefits for the calendar month prior to JTPA enrollment, during the month of enrollment, or during the month after enrollment. (A similar definition would be used to define UI recipiency status for new ES registrants, except that the three different months would be defined in relation to the month of application to the ES. Such a definition would not be sensitive to the particular ES/UI relationship in the state.) Although this does not correspond precisely to the definition used to define a UI claimant for the purposes of JTPA (i.e., is eligible for UI benefits at the time of JTPA application, and may in fact be receiving UI benefits), because very few participants are likely to begin receiving UI benefits after JTPA application, this definition should be operationally similar to the JTPA definition. The following steps could be used to obtain the necessary UI benefits data for individuals in the participant samples who enrolled in JTPA during the first quarter of PY 1985:

1. Net impact staff will provide UI staff with a working file that contains the SSN and month of JTPA enrollment for all adult men and women who enrolled in the first quarter of PY 1985 and were selected into the participant samples.

2. UI data processing staff will compare each SSN on the working file with the SSNs in their UI Benefit History file to identify individuals who received UI benefits for a compensable week of unemployment during any month from April 1985 through October 1985.

3. UI data processing staff will calculate the total amount of UI benefits received by a person during each of the seven months of interest (i.e., April through October). Monthly totals will be defined to include all checks issued for weeks in which the week ending date for which benefits were claimed is in that month, regardless of when the checks were issued. All persons who do not match in Step 2 (i.e., those who received no UI benefits during the 7-month period) will have each of their monthly totals set to zero.

4. UI data processing staff will return a data tape to the net impact project staff that includes the SSN, the month of enrollment, and the monthly amount of UI benefits issued for each month from April 1985 through October 1985 for all adult men and women in the working file in Step 1.

Because there is generally an initial lag of a few weeks before UI claimants receive their first check, to ensure that all payments for compensable weeks in October have been issued, it will not be possible to complete this task until late November or early December 1985 for the first quarter of PY 1985 enrollees.

By following steps like those listed in each of the above pre-program data collection tasks, important information will be obtained on the pre-program experiences of JTPA participants from several sources. As the information is received from the various state agencies, initial steps should be taken to process these data into a format that will be more useful for analysis purposes. Specifically, a pre-program data file should be developed for each quarterly sample of JTPA enrollees that includes the following information:

- Social Security number;
- Quarter of program enrollment (e.g., 851 for those who enrolled in the first quarter of PY 1985);
- Month of program enrollment (e.g., 7 if July, 8 if August);
- Pre-program quarterly UI wages earned (up to twelve quarterly values, beginning with the immediate pre-program quarter);
- Pre-program quarterly AFDC grants received (calculated as the sum of the monthly values in a particular quarter, and beginning with the immediate pre-program quarter through up to twelve quarters or more);
- AFDC recipient status (defined as 1 if the person received AFDC grants in the month prior to the month of JTPA enrollment, during the month of enrollment, or during the month after enrollment; 0 otherwise);

- UI benefits received during the immediate pre-program quarter (e.g., for first quarter of PY 1985 enrollees it would be calculated as the sum of the monthly values for April through June 1985);

- UI recipient status (defined as 1 if the person received UI benefit payments during the month prior to the month of enrollment in JTPA, during the month of enrollment, or during the month after enrollment into JTPA; 0 otherwise); and,

- JTPA application data.

Although the organization of the data file need not be precisely as that given above, it is particularly useful to place the JTPA application data at the end of the file. This is so that similar data extraction programs can be used for both participants and comparison group members when creating the analysis files. That is, because the same number of quarters of UI wages, AFDC grants, and UI benefits will be available for both groups, by ordering the data file in this way the same data items will appear in the same fields on the file. The only difference in the file between the two groups would be at the end where the JTPA and ES application data would be placed. This pre-program data file will include all of the information necessary for selecting the final quarterly sample of matched comparison group members as described below. Moreover, all of the pre-program characteristics of participants that will be used in the net impact model can be derived from this pre-program data file.

Before turning to a description of how to draw the comparison groups, one important feature of the pre-program data file should be noted. Specifically, because the file will have the same format for each group of quarterly enrollees, and because the pre-program data elements are defined in terms of their relationship to the quarter of enrollment (i.e., ordered beginning with the immediate pre-program quarter, and up to 12 quarters prior to enrollment, data permitting), data elements in the same fields will correspond to different calendar periods in different quarterly files. For example, although UI wages earned in April to June 1985 will be the first data item in the group of wage items for first quarter of PY 1985 enrollees (i.e., since it corresponds to the immediate pre-program quarter), it will be the fourth variable in the group of wage items for persons who enroll in JTPA in the fourth quarter of PY 1985. As such, before comparing dollar amounts in certain pre-program quarters across files, it is important to adjust for overall price changes and translate all dollar amounts into real terms. Although it would be possible to include the values of the price index to be used for this purpose directly on the data file, because it takes on the same value for all participants and comparison group members in a particular calendar quarter, we believe it is more efficient to save space on the data file and recode the variables into real terms when preparing the instructions for the analysis runs. This process is described in more detail later.

## SELECTING THE COMPARISON GROUPS

The next major data collection and processing task involves the selection of the comparison groups. As indicated in the research design above, samples of adult men and women new ES registrants will be selected on a quarterly basis. As such, this task will be performed by ES data processing staff, with guidance from the net impact project staff. Below we list steps that could be used to select appropriate comparison groups for adult men and women who enrolled in JTPA during the first quarter of PY 1985:

1. Create a list of all ES offices that are located in areas served by SDAs in the state. In most cases, this will include virtually all of the ES offices in the state.

2. Create a working file (on tape) of all individuals who were new registrants in the ES offices identified in Step 1 during the period from July 1, 1985 through September 30, 1985. The initial working file should include the person's SSN, age, sex, and indicators of whether the person is economically disadvantaged, a UI claimant, and an AFDC/welfare participant. Although the specific date of registration is not necessary for selecting the sample, it may be advisable to include it on the initial working file so that one could make sure that the extraction program worked properly.

3. Exclude from the working file all persons who are: not economically disadvantaged; age 21 or less; or who are older than the oldest individual in the quarterly participant sample. Thus, although the design allows for including JTPA participants if they are age 64 or less, if no JTPA enrollees in the first quarter of PY 1985 are older than say age 57, the age range for comparison group members would be similarly restricted.

4. Stratify the ES registrant group by sex. That is, create a subfile of adult men and a separate subfile of adult women ES registrants.

5. Using the last two digits of the SSN, select an expanded initial sample of adult men and women new ES registrants from the two separate subfiles. The size of the expanded initial samples should be sufficiently large to yield quarterly samples of comparison groups that are at least as large as the participant groups in each quarter, after adjusting for expected sample loss due to excluding ES registrants who received significant services as well as those who are or have been JTPA participants. In addition, the size of the initial samples must be large enough to ultimately yield sufficient numbers of ES registrants in each of the four cells defined by combinations of UI recipient status and AFDC recipient status, so that a comparison group with a similar distribution on these characteristics can be drawn. Because the expanded initial sample size is likely to vary considerably from state-to-state depending on individual characteristics, local economic conditions, and the state policies concerning the relationships

among the local ES, AFDC, and UI offices, it is very difficult to provide precise guidelines. As a starting point, we recommend that initial samples of 2,500 adult men and 2,500 adult women ES registrants be selected that have a similar distribution as the JTPA samples on the four cells comprising combinations of UI claimant status and AFDC/welfare participation status as defined on the ES data base (because appropriate agency data are not yet available for them). Thus, all persons on the two separate adult men and women subfiles created in Step 4 would be allocated to one of the four cells comprising combinations of UI claimant status and AFDC participation status and sampling rates would be determined for each cell to yield a random sample of 2,500 ES registrants with the appropriate distribution of characteristics. For example, suppose the sample of JTPA adult women enrollees for the first quarter of PY 1985 is comprised of 8% who were UI recipients but not AFDC recipients, 60% who were neither UI recipients nor AFDC recipients, 32% who were not UI recipients but were AFDC recipients, and that no one was both a UI recipient and an AFDC recipient. Then, sampling rates should be set to yield a sample of 2,500 adult women ES registrants with approximately 200 individuals who were UI claimants but not AFDC participants, 1,500 individuals who were neither UI claimants nor AFDC participants, and 800 individuals who were AFDC participants but not UI claimants, and no individuals who were both a UI claimant and an AFDC participant. Thus, if in the first quarter of PY 1985 there were 2,000 adult women ES registrants who were claimants but not AFDC participants, 7,500 registrants who were neither claimants nor AFDC participants, 1,600 registrants who were AFDC participants but not claimants, and 200 individuals who were both a claimant and an AFDC participant, then the sampling rates should be set at 10%, 20%, 50%, and 0 respectively, and the SSN ranges should be set accordingly (e.g., 00 to 09, 00 to 19, 00 to 49 for the first three cells respectively). Sampling rates for adult men would be set using the same approach.

6. Obtain available UI Wage Records, AFDC Grants Records, and UI Benefit History data for all ES registrants identified in Step 5 (approximately 2,500 adult men and 2,500 adult women). It is necessary to request these data at this time in order to ensure that the same number of quarters of pre-program data are obtained for comparison group members as for participants. These data should be obtained using the identical procedures used to obtain pre-program data for participants as described above.

7. Create AFDC recipient status and UI recipient status variables for all comparison group members on the the two separate subfiles using the data obtained in Step 6. It is important that these variables be comparably defined to the measures developed for participants that were described earlier. That is, comparison group members will be considered to be recipients of the particular income source if they received income from that source during the month prior to, the month of, or the month after ES registration.

8. Obtain ES services records for all individuals identified in Step 5 for the period from July 1, 1985 through December 31, 1985 (i.e., for three complete months after the end of the quarter of registration). Individuals will be excluded from the sample identified in Step 5 if they received significant ES services during this period. Specifically, all individuals who have a transaction code on their ES service record indicating that during the period from July 1, 1985 through December 31, 1985 they received a job referral, counseling, testing, job development, or participated in a job search workshop, job finding club, or other similar service that may be offered in the ES offices in the sample, will be excluded from the samples of adult men and women new ES registrants. (It should be noted that although the length of the postregistration period will vary somewhat across individuals by using a fixed end date for receiving ES services for all registrants during a particular quarter, this approach was chosen to simplify data processing and because it generates a period of exposure to ES services that is approximately four and one-half months on average, which is very similar to the average length of stay in JTPA.)

9. Compare the SSNs of potential comparison group members who remain after Step 8 with the SSNs of all current JTPA participants, as well as with lists of participants in the prior year if possible (obtained from JTPA), and exclude all SSNs that match. That is, exclude from the potential comparison group samples all individuals who are or who have recently been JTPA participants. In addition, at this point one should check to make sure that the remaining SSNs are unique, that is, that individuals are in the comparison group only once, and if any individuals are there multiple times, only their record with the earliest registration date should be retained; all others should be excluded.

10. Select from the adult men and women who remain after Step 9, a random subset that matches the distribution of participants on the four cells comprising the combination of the comparable measures of UI recipient status and AFDC recipient status. That is, the remaining SSNs on the two separate adult men and women subfiles would be allocated to the four cells comprising combinations of UI recipient status and AFDC recipient status (created during Step 7), and sampling rates would be determined for each cell to match the quarterly distribution of participants on these characteristics. Since all of the pre-program data necessary for analysis will have already been collected in Step 6, this final step should be designed to retain the maximum comparison sample size possible, rather than exclude additional cases just to reach the approximate participant sample size. Thus, using the above example, rather than setting sampling rates for each cell to generate approximately 800 matched ES registrants overall, and with approximately 64 (8%), 480 (60%), 256 (32%), and 0 registrants in the four cells, the rates would be set so that approximately 8%, 60%, 32%, and 0% of the comparison group selected are in the four cells, but retaining the maximum total sample size

> possible. For example, if after the exclusions in Steps 8 and 9, the 2,500 adult women initially selected were reduced to 1,200 in total with 100, 800, and 300 in the first three cells, the sampling rates required to obtain 800 individuals with the same distribution as the quarterly participant sample (i.e., 64, 480, and 256) would be 64%, 60%, and 85% respectively. However, by including all of the individuals in the cell with the highest required sampling rate, that is, by including all 300 ES registrants who are AFDC recipients but not UI recipients, and ensuring that they remain in the same proportion of the total sample (i.e., 32%), it would be possible to develop a somewhat larger sample of approximately 938 comparison group members. To ensure 8% of the sample in the first cell and 60% in the remaining cell, this suggests total sample sizes of 75 comparison group members who are UI recipients but not AFDC recipients and 563 who are neither UI recipients nor AFDC recipients. As such, the necessary sampling rates would be 75% for the first cell and approximately 70% for the second cell.

By following these ten steps, one could obtain samples of adult men and women new ES registrants in the first quarter of PY 1985 that are similar to the participant samples on important characteristics such as economically disadvantaged status, AFDC recipient status, and UI recipient status, and who did not receive significant employment and training assistance. By repeating these steps for the subsequent three quarters one would be able to obtain sufficient numbers of matched comparison group members for the entire program year for analysis purposes.

Although these steps could be used by most states, some of the specific steps may need to be modified to meet various states' analysis needs, data limitations, or specific circumstances. For example, states that face very good economic conditions could require considerably larger initial samples of adult men and women than the 2,500 recommended in Step 5, in order to accommodate the larger-than-expected sample loss at Step 8 due to a greater proportion receiving ES services. Because of the many factors that may affect the size of the initial samples, states will need to carefully monitor the sizes of the resulting comparison group samples to determine whether larger initial samples are required. In addition, larger initial samples may be required for states that desire greater statistical precision. As described in the evaluation guide, one can increase the statistical power of the overall net impact estimates somewhat by increasing the size of the comparison groups. Operationally, to double the expected size of the comparison groups, one could simply double the initial sampling rates used in Step 5 to select potential sample members from the four matched cells.

More significant modifications to the above steps will be necessary in states that do not have all of the required data elements in their ES MIS system. The most likely potential problems relate to the availability of measures of economically disadvantaged status and AFDC participation status. As described in Chapter 3, because of recent reductions in federal reporting requirements, states are no longer required to report the number of economically disadvantaged applicants who are registered and served by the ES. As a result, it is possible that some states no longer collect information on whether a person is economically disadvantaged at registration. In addition, although all states collect some information on whether an ES registrant is receiving public assistance at application, it may not be possible to accurately identify whether the person is receiving AFDC grants at that point. States that face such data limitations either need to modify their data collection systems to incorporate these data items or modify the steps involved in selecting comparison group members as described below.

If the ES MIS system only identifies public assistance recipients in general, and does not enable one to reliably identify AFDC recipients, then a much larger number of initial PA recipients must be selected at Step 5. The amount by which the sample of PA recipients should be increased depends on the likely proportion of AFDC recipients to total public assistance recipients in the ES registrant file. It also should be noted that this proportion is likely to vary between men and women. As an example, if approximately half of the women PA recipients on the ES file are AFDC recipients, then instead of an initial sample of 2,500 adult women ES registrants of which 800 were AFDC participants, one would instead draw an initial sample of 3,300 adult women ES registrants with 1,600 individuals who were PA participants in order to generate approximately 800 AFDC participants.

A potentially much more serious problem arises if the ES MIS does not include information on the economically disadvantaged status of ES registrants. Because virtually all JTPA participants are economically disadvantaged, it is important to develop procedures to select comparison group members who are also economically disadvantaged. In the absence of specific information on economically disadvantaged status, an alternative approach would be to exclude individuals with high pre-program earnings and then explicitly match the remaining comparison group members to participants on the basis of pre-program earnings. That is, instead of excluding all persons who are not economically disadvantaged at Step 3, one would first obtain UI Wage Records for a much expanded sample at Step 6 (perhaps up to five times as large if only 20% of ES registrants are economically disadvantaged) and exclude all persons with high earnings in the immediate pre-program period and who would certainly not be eligible for JTPA. Although the precise cut-off level is a matter of judgment and depends on the distribution of pre-program earnings in both samples, a cut-off level set at the maximum earnings of participants (separately for adult men and women) in the six months before enrollment or somewhat higher (e.g., one standard deviation higher) seems reasonable.

Although this would help to ensure similarity in terms of maximum earnings in the pre-program period, in the absence of data on economically disadvantaged status it is also desirable to match the samples more closely in terms of the time pattern and levels of pre-program earnings. For example, based on the pre-program pattern of participants' earnings, one could create specific cells that are mutually exclusive and exhaustive, and then select comparison group members from these cells to match the distribution of participants.

For example, one could aggregate quarterly earnings into annual measures for the immediate pre-program years and create the following five cells corresponding to ranges of earnings in the immediate pre-program year: $0, $1-$1,999, $2,000-$4,999, $5,000-$7,999, and $8,000 to the participant maximum. To ensure that one matched the samples well in terms of recent changes in earnings, one could also create cells to identify individuals who experienced particularly large decreases in earnings from the second pre-program year to the immediate pre-program year. For example, based on an examination of earnings changes in these years, one might define large reductions in earnings to be $2,000 or more. Then in addition to excluding individuals with high pre-program earnings, one would include an earnings stratification as part of Step 5. In this example, there would be 40 different cells representing combinations of earnings levels in the immediate pre-program year (5 cells), earnings changes from the second to the first pre-program year (2 cells), UI recipiency status (2 cells), and AFDC recipiency status (2 cells). By excluding individuals with pre-program earnings above a particular amount and matching the samples on the basis of the level and changes of pre-program earnings, one should be able to overcome most of the problems caused by the absence of information on economically disadvantaged status.[18] It should be noted, however, that to ensure there are sufficient numbers of comparison group members to fill this many cells, considerably larger initial samples may be required at Step 5.

Two other potential data issues should also briefly be mentioned. First, although the characteristics of ES registrants to be used in the steps described above are conceptually measured as of the date of registration, data obtained from the ES MIS reflect the current characteristics of the individual at the time the data are processed. That is, if the person's status changes since registration (e.g., became a UI claimant, no longer a Food Stamps recipient), the data base will be updated to reflect these changes, and an indication that the update was performed will be included on the data base. Because the initial ES samples will be drawn from one to four months after the individuals register with the ES, it is unlikely that the type and number of status changes that occur will affect the sample selection procedures. However, each state should review their own specific situation and determine whether it is important to undertake the task of "undoing" the updates to retrieve characteristics measured at registration.

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[18] A potential evaluation problem remains in that individuals in the comparison group with consistently zero earnings in the pre-program period are much more likely to have been in uncovered employment than otherwise similar participants who have also been identified to be economically disadvantaged. Because such comparison group members are not economically disadvantaged, if they become employed in the covered sector, they are likely to have much higher earnings than participants in the post-program period, which would bias downward the net impact of JTPA. Because no data are available to determine for sure that individuals were working in uncovered employment (as opposed to not working at all), the best one can do is examine to what extent the net impact of JTPA varies by pre-program earnings, and, if the impacts are large and negative for individuals who had zero earnings in the pre-program period, this is likely to be the result of a selection bias. In this case, it may be better to exclude cases with zero earnings in the pre-program period from the analysis to obtain a more reliable estimate of the net impacts of JTPA. It should be noted, however, that the resulting net impact estimates would only be generalizable to participants with positive earnings in the pre-program period.

Finally, it should be noted that the ability to overcome potential contamination of the comparison groups is in part determined by the availability of data on current and prior JTPA participants. As indicated in Step 9, it is desirable to exclude all ES registrants who are currently participating in JTPA or who participated in JTPA in the previous year. To implement this step requires that JTPA data processing staff create a cumulative file of all persons (SSNs) who participate in JTPA programs and their dates of participation. If it is not possible to implement this step, then minor adjustments must be made to the overall net impact results to account for potential contamination bias as described in Chapter 5.

## OBTAINING PRE-PROGRAM DATA FOR THE COMPARISON GROUPS

As indicated above, although the steps to be used in obtaining various data for the pre-program period for comparison group members are identical to the steps used for participants, the timing and magnitude of the task differs considerably. That is, to ensure that the same numbers of quarters of pre-program data are obtained for both participants and the final comparison group members, as indicated in Step 6 above, it is necessary to obtain these data for much expanded potential comparison group samples (i.e., approximately three times as large as the participant samples or larger). Moreover, because Step 5 in the comparison group sample selection cannot be completed until after the AFDC Grants and UI Benefit History data are obtained and processed for participants (approximately early December 1985 for first quarter of PY 1985 enrollees), it is important that Step 5 be completed as soon as possible after that time so that the pre-program data for the expanded samples selected can be obtained by the end of December 1985 or else the comparison groups may end up with fewer quarters of pre-program information. It should be noted that the same steps that were followed to overcome potential data reliability problems for participants that were discussed above must also be followed in obtaining pre-program data for the comparison groups.

By the time the final comparison group members have been selected for a particular quarter (i.e., after Step 10), nearly all of the necessary pre-program information for analysis purposes will have been obtained. The only exception is the remaining ES application information that was not obtained in Step 2 of the comparison group sample selection process. Although it is, of course, possible to obtain all of the application data at Step 2, because the information is relevant only for the relatively small subset included in the final comparison group samples, it is somewhat inefficient to include these variables on the working files throughout. However, depending on the accessibility of the ES data system and the timing of demands placed on ES data processing staff, it may be preferable to obtain all of the ES characteristics at the time the initial expanded sample is selected, but only include the few variables needed on the working file for Step 2, and then subsequently merge the ES characteristics data to the individual records for those persons included in the final analysis samples. This would avoid having to make an additional data request of ES each quarter. To determine the most appropriate procedures in a given state it will be necessary for the net impact staff to meet with ES data processing staff.

After all of the pre-program information has been obtained, a pre-program data file will be developed for each quarterly sample of ES registrants included in the final comparison groups. As discussed earlier, we recommend that the pre-program data file be organized similarly to the participant pre-program data file and include:

- Social Security number;
- Quarter of ES registration (e.g., 851 for those who are new registrants in ES offices in the first quarter of PY 1985);
- Month of ES registration (e.g., 7 if July, 8 if August);
- Pre-program quarterly UI wages earned (up to 12 quarters, beginning with the immediate pre-program quarter);
- Pre-program quarterly AFDC grants received (calculated as the sum of the monthly values in a particular quarter, and beginning with the immediate pre-program quarter through up to twelve quarters or more);
- AFDC recipient status;
- UI benefits received during the immediate pre-program quarter (e.g., for first quarter of PY 1985 enrollees it would be calculated as the sum of the monthly values for April through June 1985);
- UI recipient status; and,
- ES registration data.

All of the pre-program characteristics of comparison group members that will be included in the net impact model can be derived from this pre-program data file.

## PREPARING A PRE-PROGRAM ANALYSIS FILE

Once the quarterly samples of participants and comparison group members have been selected and the pre-program quarterly data files have been developed, the next task involves the development of pre-program analysis files for adult men and women. This task involves the merging of quarterly data files for participants and comparison group members, data editing, the creation of analytic variables, and procedures for handling cases with missing data. Once the pre-program analysis files are developed, it will be possible to implement several of the analytic procedures described in Chapter 5 and investigate the comparability of the participant and comparison groups in the pre-program period. Below we describe in more detail the various steps involved in developing pre-program analysis files for adult men and women.

### Merging Quarterly Files

The first step involves the merging of the four quarterly participant data files and the four quarterly comparison group data files into annual participant and annual comparison group files. Because the formats for each of the four data files should be identical, the merging process should be straightforward. If it has not been done previously, at this point the annual files should be checked to ensure that the samples of comparison group members and participants are each unique (i.e., that no individual is on one of the files more than once) and that there is no overlap between individuals in the two files (i.e., that a comparison group member is not also a JTPA participant). Because these checks will be made by comparing SSNs either within or across files, this process would be more efficient if the files were first sorted by SSN.

### Editing the Data Files

The next step involves implementing procedures to clean or edit the raw data. This process is basically a check on the quality and consistency of specific data items. Although a considerable amount of cleaning and editing will have been performed by the respective ES and JTPA data processing staffs as part of their normal procedures, it will be useful for the net impact data processing staff to conduct certain edit checks to familiarize themselves with the different files and to check the quality of the data. The first type of edit check involves comparing the results of a simple frequency distribution on all variables in each of the annual files with a range of acceptable values. For continuous data elements (e.g., UI wages, UI benefits, AFDC grants), a range of acceptable values should be created that is based on common sense and incorporates rough estimates of the maximum amounts that can be received from certain programs in the state. For example, one could recode the UI benefits received in the immediate pre-program quarter into several ranges such as \$0; \$1-\$499; \$500-\$999; \$1,000-\$1,499; \$1,500-\$1,999; \$2,000-\$2,499; \$2,500-\$2,999; and \$3,000 or more. Because the highest category requires someone to have received nearly \$250 per week in UI benefits, which is not possible in most states, any such values should be cause for additional investigation with UI data processing staff to ensure that the appropriate steps were followed in creating the quarterly measure of UI benefits.

For data items obtained from ES or JTPA application forms, the range of acceptable values can be specified completely. That is, if handicapped status is coded as 1 for yes and 2 for no, then any values other than 1 or 2 are clearly errors that likely occurred in entering (keypunching) the data into the MIS. Thus, by reviewing a simple frequency distribution of all of the data items on each of the files and comparing the values to allowable codes, it will be possible to readily recognize clear data recording errors. Unless such errors can be readily corrected using other information on the file, they should be set to a common missing data code (e.g., -9) and dealt with as part of the procedures for handling missing data.

Although some errors are obvious by inspection of a single data item, other errors may not be apparent except when viewed in combination with another data item. Thus, as a second edit check, we recommend that limited cross tabulations be conducted of certain items to identify additional potential data quality problems. Although such edit checks are usually more useful and definitive when comparing interrelated items within a given survey, it may be possible to identify some aberrant cases in these agency data that should be excluded from the analysis. For example, cross tabulations of age by education could identify 22 year-old individuals with 19 years of education, which is quite unlikely. It would also be of interest to cross tabulate earnings and AFDC grants received in the same pre-program quarters. Individuals with large values of earnings and AFDC payments in a given quarter may be indicative of data errors, or possibly fraud. In addition, it may be informative to cross tabulate pre-program UI wages with UI benefits in the immediate pre-program period. Because individuals must have worked in covered employment in the preceding year to year and one-half to receive UI benefits, such a cross tabulation could help to identify individuals who worked in uncovered employment (i.e., positive UI benefits but zero UI wages in the prior four to six quarters) that would be of use for the net impact model.

## Creating Analytic Variables

Once the data have been cleaned to the extent possible, the next step involves the creation of analytic variables. In developing variables for the pre-program analysis file, it is very important that the variables created be comparably measured for participants and comparison group members. For variables that are derived from a common source (e.g., AFDC Grants Records, UI Wage Records, UI Benefit History data), data comparability should not be a problem. However, measures of personal characteristics will be obtained from both the ES and JTPA MIS, and differences in the ways in which questions are asked or answers are recorded can present major obstacles to creating comparable variable definitions. In addition to the many variables that will be included on the core pre-program analysis file, while conducting specific analysis tasks certain types of data may be added, general data transformations may be performed, and procedures for handling cases with missing data may be implemented. Below we describe some of these steps involved in developing analytic variables for a pre-program analysis file.

As indicated above, when the source of information for participants and comparison group members is the same, the creation of analytic variables is very straightforward. In fact, the variables of this type that we recommend be included in the pre-program analysis file will already be available on the pre-program annual data files at this point and, as such, only an extraction program is necessary. Specifically, the following variables should be extracted from the pre-program annual data files for participants and comparison group members:

- Pre-program quarterly UI wages earned;
- Pre-program quarterly AFDC grants received;
- AFDC recipient status;
- UI benefits received during the immediate pre-program quarter; and,
- UI recipient status.

Assuming that three years of pre-program information is available for both UI wages and AFDC grants, then 27 variables will be included on the analysis files for adult men and women in this step.

In extracting these variables from the pre-program annual data files, the only modification required is to convert the variables that are expressed in dollar amounts into constant dollar values (i.e., adjust for inflation). Specifically, using quarterly values of the BLS Consumer Price Index for all Urban Consumers, one would deflate (divide) the values of the variables expressed in nominal dollar terms by the value of the price index in the same calendar quarter, and create measures of real earnings and real AFDC grants received in each pre-program quarter and real UI benefits received in the immediate pre-program quarter. It should also be noted, that although more aggregated measures of these variables (e.g., semiannual, annual) may be useful for some analysis purposes, we recommend that such aggregation be performed through recoding variables while running specific analysis programs, rather than expanding the basic analysis file unnecessarily.

Although the process involved in creating consistent measures of analytic variables is straightforward when the data sources involved are the same for participants and comparison group members, more problems can be anticipated in developing measures of personal characteristics that must be obtained from different agency data bases. The problems that can arise are most obvious when clearly different questions are asked, or when the answers are recorded in different ways. Moreover, even when the questions and the response codes appear to be the same, the information collected may correspond to slightly different concepts due to differences in formal and informal staff instructions and training. Because of these potential problems, it is very important that the net impact staff carefully review the application forms to both the ES and to JTPA and the corresponding handbooks that provide instructions for recording answers to each question. Any issues that cannot be clearly resolved through reviewing these materials should be discussed in detail with appropriate agency staff.

After one has obtained a clear understanding of the personal characteristics in the two data bases, it will then be possible to create comparable analytic variables. Because the application forms for JTPA and the ES will differ from state to state, the precise problems that must be confronted will differ as well. In general, we would expect no real problems in creating comparable measures of age, race, or veteran status. Provided both programs code occupation in the same way (i.e., Dictionary of Occupational Titles), there should also be no problem in creating comparable measures of occupation. In some cases, it will not be possible to create comparable variables at all, because one of the application forms will not obtain data related to the characteristics of interest. For the State of Washington, because of limitations in either the ES or JTPA MIS, this currently includes characteristics such as marital status, family size, presence of dependent children, offender status, limited English-speaking ability, and hours worked per week on previous job.

Characteristics such as education and handicapped status are likely to fall between these two extreme situations. For example, in Washington State, the ES application form records the highest grade of schooling completed (from 0 to 19 years), whereas the JTPA application form records an individual's education status in terms of one of the following four codes: (1) school dropout; (2) in school (high school or less); (3) completed high school or received GED; and (4) currently attending or has attended schooling programs beyond high school. In terms of handicapped status, although both programs obtain some information on whether a person has either a physical or mental disability, for JTPA the disability must constitute a substantial barrier to employment, whereas for the ES, the definition is more general and refers to an impairment that substantially limits one or more of the major life activities. Although it is not possible to completely overcome the conceptual differences in handicapped status, it is possible to define consistent measures of education. Specifically, three dummy variables can be reliably created: (1) a dummy for not a high school graduate (1 if response code 1 or 2 for JTPA enrollees and if the highest grade completed is 0 through 11 for ES registrants; 0 otherwise); a high school graduate dummy (1 if code 3 for JTPA participants and if the highest grade completed is 12 for ES registrants; 0 otherwise); and a post high school dummy variable (1 if code 4 for JTPA participants and if the highest grade completed is 13 through 19 for ES registrants; 0 otherwise). Following procedures such as these, one should be able to obtain comparable measures of personal characteristics for participants and comparison group members.

The list of personal characteristics that can be comparably defined for participants and comparison group members is likely to vary somewhat from state to state depending on state-specific data collection and recording practices. In general, we expect that one will be able to construct the following independent variables for both groups in all cases:

- Age (in years);
- Race

  - White dummy variable
  - Black dummy variable
  - Hispanic dummy variable
  - American Indian/Alaskan Native dummy variable
  - Asian/Pacific Islander dummy variable;

- Education

  - Not a high school graduate dummy variable
  - High school graduate dummy variable
  - Post high school graduate dummy variable;

- Veteran dummy variable; and,

- Occupation (primary DOT code of previous job).

As discussed above, depending on data availability and recording practices, it may also be possible to obtain consistent measures of handicapped status, marital status, family size, presence of preschool aged children, etc. Because of the important role personal characteristics play in the net impact model, every effort should be made to take full advantage of the data collected by JTPA and ES and create comparable variables whenever possible.

The steps described above should result in trim pre-program analysis files of 35 to 40 variables. However, it should be noted that in attempting to minimize the size of the pre-program analysis files, certain variable creation activities have been set aside to be performed as part of the preparation activities for specific analysis tasks. That is, some variables that do not vary across individuals within a site or a given quarter are more efficiently created as part of the analysis runs, and other variables will be created through recoding of variables already on the file. Examples of variables that do not vary across individuals within a given site or quarter that should be added at this point include:

- Set of four quarterly dummy variables for enrolling (registering) in JTPA (ES) in a particular quarter of the program year (based on month of enrollment/registration);

- Urban location dummy variable (based on SDA and local ES office locations); and,

- Quarterly local unemployment rates (based on quarterly rates as they apply to the areas served by particular SDAs and ES offices).

It should be noted that although we recommend that these variables be created as part of specific analysis runs in order to minimize the size of the analysis files, some states may prefer to create these variables and attach them to the pre-program analysis records for each person. This will in part depend on the computer facilities being used for data processing and analysis tasks and the importance of having trim analysis files that can be downloaded for analysis on a personal computer. Although the first two examples above are very straightforward, states may find it particularly awkward to use a separate program to determine the unemployment rate for persons in a given site in a given quarter and then attach the data based on location codes. If states choose to create the unemployment rates separately and add them to the pre-program analysis files for each person, it should be noted that this will add up to 12 variables to the pre-program analysis files. Whichever process is used, similar steps will have to be followed in creating post-program unemployment rates.

Other variable creation activities can also be anticipated as part of the specific analysis runs. These activities will generally involve recoding variables that are already on the file to create new variables that enable one to investigate a specific issue. Such variable creation activities include:

- Create sets of dummy variables from continuous analytic variables.
    - Create one-digit DOT code dummy variables from the more detailed information on the analysis files. By recoding the primary DOT code into appropriate subgroups, one would create separate occupation dummy variables for: professional, technical, and managerial workers; clerical and sales workers; service workers; agricultural, fishery, and forestry workers; processing workers; machinists; benchworkers; structural workers; and miscellaneous.
    - Create separate age dummy variables (e.g., for ages 22 to 24, 25 to 34, 35 to 44, 45 to 54, and 55 and older).
    - Create dummy variables for whether employed in a particular quarter or whether received AFDC during a particular quarter based on whether the income from the particular source was greater than 0 in that quarter.
- Aggregate quarterly variables into semiannual or annual measures of earnings or AFDC grants received. Because the quarterly variables will already be expressed in real terms, this merely involves summing the appropriate quarterly values.
- Construct interaction terms (i.e., multiply analytic variables together). Examples of such variables include age squared and different race dummy variables multiplied by age or by different education dummy variables. See Chapter 5 for a discussion of the types of interaction terms that will be investigated in the net impact models.

### Procedures for Handling Cases with Missing Data

In general, we do not expect that the analytic variables to be created will suffer from missing data problems. This is in part because of the data editing and cleaning that will already have been performed by ES and JTPA data processing staff as part of their routine procedures. In instances where missing data are confronted in independent variables, however, we believe it is preferable to "patch" the variable in question rather than exclude all cases that have missing data on any relevant variable. Below we briefly describe the recommended approach for "patching" cases with missing data on independent variables to be included in the net impact model.

As described in the evaluation guide, there are several alternative procedures that could be used to create substitute values of independent variables for persons who for one reason or another do not have information on that variable (e.g., age may be missing because the agency staff person forgot to ask date of birth). Because we do not expect that missing data will be much of a problem, however, we do not believe it is necessary to develop an elaborate system to deal with such problems. As a result, we recommend that mean values of the independent variables--calculated separately for participants and comparison group members, and, of course, separately for adult men and women--be used for cases with missing data on a specific variable. Thus, as part of the initial analysis tasks, one should calculate the means of all independent variables separately for participants and comparison group members on the analysis files and prepare recode statements that set the value of a variable equal to the appropriate mean whenever it is missing. In addition, if there are likely to be differences in the independent variables among the quarterly samples, one should consider using means calculated separately by quarter of enrollment/registration to capture trends in these variables over time.

## OBTAINING AND PROCESSING DURING PROGRAM AND POST-PROGRAM OUTCOME DATA

In order to estimate the net impact of JTPA on earnings and AFDC grants received, it is necessary that postenrollment UI Wage Records and AFDC Grants Records be obtained for both the participant and comparison groups and that appropriate variables be constructed and merged onto the pre-program analysis file. In addition, as indicated earlier, UI Wage Records must be obtained again for the immediate pre-program quarter for all participants and comparison group members to correct for potential measurement error problems due to obtaining data "too early" for that period. Below we briefly outline the steps involved in obtaining and processing these data.

No problems are anticipated in collecting quarterly UI Wage Records in the postenrollment period. In order to minimize the burden to UI data processing staff, provided the state maintains a reasonably long history of UI Wage Records, the necessary information can be obtained from a single request made at the very end of the data collection process. For example, assuming that length of stay is no longer than six months for all (or almost all) JTPA participants, then the first complete post-program quarter for the last quarter of enrollees in PY 1985 (i.e., those who enrolled between April 1, 1986 and June 30, 1986) will cover the period from January 1, 1987 to March 31, 1987. Because of the three-month time lag in data availability, this information cannot be obtained until July 1987. In states that retain only six quarters of UI Wage Records, the data available in July 1987 will cover the six calendar quarters beginning in October 1985. As such, although this would enable one to obtain all of the necessary post-program wage data as well as update the earnings variable for the immediate pre-program quarter for both the third and fourth quarter PY 1985 samples of participants and comparison group members, it would not enable one to update the immediate pre-program UI wages for first or second quarter enrollees/registrants. In states that retain at least eight quarters of UI Wage Records it will be possible to obtain the required data from a single request made two years after the month in which the first quarter of individuals were enrolled (i.e., July 1987 for PY 1985 enrollees); in states that retain only six quarters of UI Wage Records an intermediate request will have to be made no later than 18 months after the month in which the first quarter of individuals were enrolled.

After completing this process, one will have obtained eight quarters of UI Wage Records for participants (comparison group members) who enrolled (registered) during the first quarter of PY 1985, seven quarters of data for those who enrolled/registered during the second quarter, six quarters of data for those who enrolled/registered in the third quarter, and five quarters of data for those who enrolled/registered during the last quarter of PY 1985. Each set of quarterly earnings data will include one quarter to update the values in the immediate pre-program quarter, one quarter for the actual quarter of enrollment/registration, two subsequent quarters of data that are likely to include in-program earnings for some participants, and varying numbers of quarters of earnings records (from one to four) that can be considered to be post-program. After the quarterly records have been obtained, they should be divided by the consumer price index for the appropriate quarter to create real earnings variables. The variables should then be merged to the pre-program analysis files for adult men and women, with the updated immediate pre-program variable replacing the initial value, and with seven additional quarterly earnings variables included on each record. These seven variables will correspond to the real quarterly earnings during the actual quarter of enrollment, real earnings during each of the two subsequent quarters (that will usually correspond to during-program earnings), and four quarters of post-program real earnings. It should be noted that although the seven variables will be created for all individuals to maintain the rectangular nature of the analysis file, missing data codes (e.g., -9's) will be placed in the real earnings variables for post-program quarters for which data are not currently available. Thus, for example, the second, third, and fourth post-program quarters will all have -9's for those who enrolled/registered during the fourth quarter of PY 1985. By setting these values to missing data codes it will help to ensure that one does not inadvertently include them in the analysis.

With regard to collecting monthly AFDC Grants Records during the in-program and post-program periods, depending on the state data system it may be preferable to obtain such information on an ongoing basis, rather than only once at the end of the data collection process. For example, for PA data systems such as those in Washington State in which the Recipient History File does not enable one to identify whether a person is in the particular assistance unit throughout the period of interest, it would be preferable to obtain the information on an ongoing basis each month to minimize measurement error in this key outcome variable. As discussed above, the initial request for pre-program AFDC grants data made in November 1985 for first quarter of PY 1985 enrollees, would cover the period from approximately July 1982 through October 1985. For these participants and comparison group members, it will then be necessary to obtain monthly records from November 1985 through March 1987 in order to create quarterly variables of AFDC grants received for the analysis. To obtain this information on an ongoing basis would require that for each of the next seventeen months, AFDC data processing staff would compare the SSNs of first quarter participants and comparison group members with the list of SSNs in assistance units that received AFDC payments during the month. The actual values of monthly grants would be included for SSNs that matched, and zeros would be included for those SSNs that did not match. It should be noted that the list of SSNs to be searched will grow every 3 months as participants and comparison group members who enrolled in the second, third, and fourth quarters are included.

Although collecting postenrollment AFDC grants data on an ongoing basis overcomes the major data quality issues described earlier, it imposes a considerable burden on PA data processing staff. If it is not possible to collect the data on an ongoing basis, then the Recipient History File will have to be used, and adjustments made for possible data base limitations discussed earlier to the extent possible (e.g., problem caused by assistance units that are receiving AFDC but classified as Food Stamps cases). Using this approach, it is likely that a single request made at the end of the data collection process would provide all of the necessary monthly information.

After the monthly data are obtained, quarterly measures of real AFDC grants received would be created and merged to the pre-program analysis files for adult men and women. Once again, to maintain the rectangular nature of the analysis files, a common number of postenrollment quarterly values will be created for all individuals on the file, and -9's will be placed in post-program quarters for later enrollees for whom the data are not yet available.

## OBTAINING AND PROCESSING JTPA MIS DATA

The final major data collection and processing task concerns obtaining and processing JTPA MIS data on program experiences. Because individuals can potentially participate in multiple activities, it appears that most state JTPA data systems will have a program activity file in which a given individual may have multiple records. In order to create consistent analytic variables to represent the program services received, it will be necessary for the JTPA data processing staff to extract the program activity records for all SSNs in the four quarterly samples of JTPA participants. Depending on the archiving procedures followed in a given state, it may be possible for these data to be obtained from a single request made at the end of the data collection process (i.e., around January 1987 for all PY 1985 enrollees). If, however, data on PY 1985 enrollees who terminate within the program year are purged at the end of the program year, it will be necessary to also obtain the program activity records for such individuals before the records are purged (i.e., before June 30, 1986).

After the program activity records are obtained, the next step involves the creation of specific variables to represent the services received by JTPA participants. As discussed in Chapter 2, the specific treatment variables to be created will in large part be determined by the structure and content of the JTPA MIS. In most cases, however, it should be possible to create the following variables:

- Participant dummy variable (1 for JTPA participants; 0 otherwise);
- Classroom training dummy variable (1 if CT participant; 0 otherwise);
    - Remedial education and basic skills dummy variable (1 if CT program in remedial education and basic skills; 0 otherwise);
    - Specific occupational skills training dummy variable (1 if CT program in specific occupational skills; 0 otherwise);
- On-the-Job-Training dummy variable (1 if OJT participant; 0 otherwise);
- Job search assistance dummy variable (1 if JSA participant; 0 otherwise);
- Other activity dummy variable (1 if participated in other activity; 0 otherwise);
- Multiple activity dummy variable (1 if individual participated in combinations of CT, OJT, JSA, and other; 0 otherwise);
- Occupation of training dummy variables based on one-digit DOT code;
    - Professional, technical, and managerial dummy variable (1 if 3-digit DOT code is less than 200; 0 otherwise);
    - Clerical and sales dummy variable (1 if 3-digit DOT code between 200 and 299; 0 otherwise);
    - Service dummy variable (1 if 3-digit DOT code between 300 and 389; 0 otherwise);
    - Agricultural, fishery, and forestry dummy variable (1 if 3-digit DOT code is between 400 and 461; 0 otherwise);
    - Processing dummy variable (1 if 3-digit DOT code is between 500 and 599; 0 otherwise);
    - Machine trades dummy variable (1 if 3-digit DOT code is between 600 and 699; 0 otherwise);
    - Benchwork dummy variable (1 if 3-digit DOT code is between 700 and 799; 0 otherwise);
    - Structural dummy variable (1 if 3-digit DOT code is between 800 and 899; 0 otherwise);
    - Miscellaneous dummy variable (1 if 3-digit DOT code is between 900 and 979; 0 otherwise);

- Length of program participation in weeks (calculated from enrollment and termination dates);

- Completer dummy variable (1 if participant completed JTPA program activity; 0 otherwise); and,

- Number of hours of training per day.

After these variables have been created, they will be merged to the pre-program analysis files for participants, and zeros will be entered for comparison group members for all of these variables.

As indicated in Chapter 3, we recommend that participants who have missing data on key JTPA treatment variables be excluded from the analysis samples, provided the reason the items are missing is random. To make this determination, it will be necessary to compare the characteristics of participants who have missing data on the variables described above with the characteristics of participants who have data on the variables. For example, one should compare the age, race, and education of individuals in the two groups to determine if there are major differences. Moreover, if in the process of collecting data on program experiences one also obtained information on placement status at termination, it would be useful to compare participants on their placement status at termination to get some sense as to whether having missing data is systematically related to the impact of the program. If participants with missing data on JTPA experiences are equally likely to be placed in jobs following the program as participants with complete data, this would provide additional confidence that the validity of the analysis will not be compromised by excluding such individuals.

## CREATING NET IMPACT ANALYSIS FILES

Once the above data collection and processing tasks have been completed, the final task involves the merging of JTPA analytic variables and during-program and post-program outcome variables to the pre-program analysis files. This will result in the creation of two net impact analysis files, one for adult men, and one for adult women. The final analysis files will include the necessary identifying information (SSN, quarter and month of enrollment/registration), all of the variables from the pre-program analysis files, the outcome variables, and the variables representing services provided by JTPA. These analysis files will be able to support all of the analysis tasks described in the next section.

# CHAPTER 5
# DATA ANALYSIS PLAN

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**Examining the Adequacy of the Comparison Groups—Obtaining Evidence on Selection Bias**

**Estimating the Average Net Impact of JTPA Programs**

**Obtaining Net Impact Estimates for Various Subgroups**

**Obtaining Net Impact Estimates by Program Length of Stay**

**Adjustments for Potential Data and Design Deficiencies**

# CHAPTER 5. DATA ANALYSIS PLAN

The final step in developing a model to obtain valid estimates of the net impacts of JTPA programs on the earnings and welfare dependency of adult men and women involves the specification of a data analysis plan. In particular, a data analysis strategy must be developed for examining the adequacy of the comparison groups selected, and for using the comparison groups to estimate the net impacts of JTPA on participants' post-program outcomes. In this section, we describe an overall estimation strategy for obtaining valid estimates of the net impacts of JTPA.

Before describing the details of the analysis plan, we would like to emphasize that the recommended approaches should be quite accessible to all states interested in conducting net impact analysis. For example, all of the analysis techniques to be used are contained in standard statistical software packages (e.g., SAS, SPSS) that should be readily available and familiar to state-level analysts. In addition, after some initial data processing on a mainframe computer, it may be possible to download the analysis files to hard disks that can be accessed by mini-computers. This will minimize the computer resources required to conduct the analysis.

An overview of the key elements of the data analysis plan is provided in Exhibit 4. In the remainder of this section we describe the steps involved in implementing this plan in more detail. First, we describe an analysis strategy for examining the adequacy of the comparison groups selected to get a better understanding of the likely direction and magnitude of selection bias. We then describe a statistical model that can be used to estimate the average net impacts of JTPA and the impacts for important subgroups. The section concludes with a discussion of potential adjustments for certain data and design deficiencies.

## EXAMINING THE ADEQUACY OF THE COMPARISON GROUPS--OBTAINING EVIDENCE ON SELECTION BIAS

As described in Chapter 3, our proposed approach involves a comparison of the post-program labor market experiences of a sample of JTPA participants with the experiences of a sample of ES registrants who do not receive ES services. If the samples of JTPA participants and ES registrants are similar on both measured (e.g., age, race, education) and unmeasured (e.g., attitude toward work, motivation) characteristics, then valid inferences about the impacts of JTPA programs can be drawn from such comparisons. However, whether an individual participates in JTPA or registers with the ES is likely to depend on both individual and agency decisions. For example, JTPA participants must decide to apply to the program, they must meet

Exhibit 4

## OVERVIEW OF DATA ANALYSIS PLAN

- Examine adequacy of comparison groups using analysis techniques such as differences in means, differences in distributions, and multiple regression analysis. The adequacy of the comparison groups will be judged in terms of three criteria:
    - Similarity of participant and comparison groups on measured characteristics (e.g., age, race, education);
    - Similarity of participant and comparison groups on pre-program earnings and AFDC grants; and,
    - Similarity of the relationships between pre-program earnings (and AFDC grants) and measured characteristics for participants and comparison group members.
- Estimate average net impacts of JTPA for adult men and women using an autoregressive model. Net impacts will be estimated for four post-program outcome measures (earnings, whether employed, AFDC grants, and whether an AFDC recipient) in each of three different periods (three months, six months, and twelve months).
- Estimate net impacts of JTPA on the various outcome measures for important subgroups of adult men and women. Although autoregressive models will primarily be used to estimate subgroup impacts, in some cases instrumental variable techniques should be considered. The subgroups of interest include:
    - Participant characteristics such as race/ethnicity, age, education, and welfare recipient status for women;
    - Program activities such as CT, OJT, JSA, and other; and,
    - Program length of stay.
- Adjust net impact estimates to the extent possible for data/design deficiencies:
    - Contamination of the comparison groups;
    - Uncovered earnings; and,
    - Selection bias.

certain legislated eligibility criteria, they must be selected by the agency for program participation and assigned to a program activity, and they must decide to accept that assignment and enroll in the program. Although ES registrants do not have to meet any formal eligibility criteria, certain individuals (e.g., those receiving benefits from government programs such as UI) are required to register with the ES, and some offices follow selective registration policies. Furthermore, whether an ES registrant receives ES services depends on several factors, including the availability of suitable job openings and the person's qualifications and persistence. Because of these various selection processes, it is unlikely that the resulting samples of JTPA participants and ES registrants who do not receive services are truly equivalent on both measured and unmeasured characteristics. This is the issue of selection bias.

It is important to recognize that virtually all nonexperimental approaches will contain a certain amount of bias. That is, the formal conditions required to ensure unbiased estimates of program impacts are not likely to be met, even if one had extensive data on the characteristics of program participants and comparison group members. This is particularly true for the proposed research design, which does not involve detailed survey data and, as a result, the amount of information available to control for selection bias is limited. As a practical matter, therefore, one should not focus on the fact that the two groups are not identical, but identify the major dimensions on which the groups differ, and determine the extent to which the net impact estimates are likely to be sensitive to those differences.

As indicated in Exhibit 4, and as discussed in detail in the evaluation guide, we recommend that three different criteria be used to judge the adequacy of the comparison groups selected:

- Similarity of the participant and comparison groups on measured characteristics (e.g., age, race, education);
- Similarity of the participant and comparison groups on pre-program earnings and AFDC grants; and,
- Similarity of the relationships between pre-program earnings (and pre-program AFDC grants) and measured characteristics for participants and comparison group members.

Although these criteria are the traditional ones that have been used to judge the adequacy of nonexperimentally-derived comparison groups, as we discuss in the evaluation guide they are necessary, but not sufficient, conditions for the comparison groups to overcome the problem of selection bias. As such, even if the comparison groups selected generally meet these three criteria, this should not be interpreted as definitive evidence of no selection bias. With this caution in mind, below we briefly outline some analyses that can be performed for each of the quarterly samples and for the annual samples as a whole to see whether these criteria are met, and if not, that will identify the extent/types of differences between the groups that must be kept in mind when interpreting the net impact results.

The first criterion that we recommend be used to judge the comparability of the participant and comparison groups is the similarity of the two groups on measured characteristics at enrollment/registration. It is particularly important to compare the participant and comparison groups on available measured characteristics that are known to affect earnings and AFDC grants. For example, it would be particularly useful to determine to what extent the two groups differ on age, race, education, occupation, and handicapped status, and other relevant personal characteristics that are comparably measured for both groups. Using standard software packages, one would compare the means and the distributions of these measured characteristics for participants and comparison group members, separately for adult men and women in each of the four quarterly samples and in the overall program year sample.[19] For example, one would compare the proportion of the participant and comparison groups in these samples that were white, black, Hispanic, American Indian/Alaskan Native, and Asian/Pacific Islander. One would also compare the average age of individuals in the two groups and the proportions that were in different age ranges (e.g., 22-24, 25-34, 35-44, 45-54, 55+) to identify differences in the age distributions that may not be reflected in the mean age comparisons. Because the output from these standard software analysis packages normally includes the results of t-tests and Chi-Squared tests for formally testing the equivalence of the means and distributions of variables in two samples, it is straightforward to compare the similarity of the participant and comparison groups on all measured characteristics.

We also recommend that similar analyses be conducted across JTPA program activities. That is, one should not only compare the characteristics of participants to the characteristics of comparison group members, but one should also compare the characteristics of participants across program activities (e.g., CT, OJT, JSA). This would provide some indication of the additional selection bias that could arise in estimating separate net impacts by program activity. For example, if it was determined that more advantaged individuals were being sent to OJT, the net impacts of this program activity would be somewhat inflated because of this assignment process. That is, the observed positive relationship between earnings and the OJT treatment would not be entirely causal, but in part reflect the fact that OJT participants were more advantaged (i.e., more motivated). On the other hand, if there were relatively few differences in measured characteristics by program activity, this evidence, in combination with other tests described below, would provide some confidence that no additional selection biases would be introduced in deriving estimates of the net impacts by program activity.

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[19] It would also be possible to estimate an OLS linear probability model of the likelihood of participating in JTPA to determine the major differences between the two groups. That is, one would estimate a regression equation with the dependent variable equal to 1 for JTPA participants and 0 for comparison group members, and the independent variables would be all measured characteristics included in the net impact model described later in this section. This approach has the advantage of estimating the independent influence of each measured characteristic, while controlling for the influence of all other characteristics, which eliminates the confounding effects of other variables that may be present when comparing mean characteristics. That is, a comparison of mean characteristics could indicate, for example, that JTPA participants are more likely to be minorities and less educated, whereas the regression approach would account for the differences in education by race and could reveal that, after adjusting for differences in race, there are no differences between participants and comparison group members in terms of education levels.

The second criterion we recommend be used to judge the adequacy of the comparison groups is the similarity of the pre-program earnings and AFDC grants of participants and comparison group members. This involves a test of whether there is a significant difference in the pre-program earnings and AFDC grants of the two groups, controlling for measured characteristics. Such a test provides valuable evidence on whether the two groups are comparable on the basis of the lagged dependent variables (after controlling for measured characteristics) or, in other words, whether there are differences in the outcome variables between the groups in the pre-program period that are due to unmeasured characteristics. If there are any differences in adjusted pre-program earnings or AFDC grants between the two groups, then this analysis will also provide some hints as to the direction and magnitude of the selection bias. For example, the extent to which JTPA participants have larger (smaller) adjusted pre-program earnings than ES registrants provides some indication as to whether they are more (less) advantaged on the basis of unmeasured characteristics. Moreover, the size of the estimated difference is a reasonable estimate of the amount by which the net program impacts could be overstated (understated) if the difference persisted in the post-program period.

To formally test for differences in the pre-program earnings and AFDC grants of participants and comparison group members, one would estimate ordinary least squares regression equations (separately for adult men and women) with pre-program earnings and AFDC grants as the dependent variables. As discussed in the evaluation guide, multiple regression is a technique that estimates the independent influence of each characteristic on a particular dependent variable, controlling for the influence of all other characteristics in the equation. For example, differences in earnings across individuals may result from differences in education and other personal characteristics (e.g., age, race), as well as differences in local unemployment conditions. The regression technique controls for the influence of local unemployment conditions and other personal characteristics and estimates the independent influence of education (and all other factors) on earnings. All standard software analysis packages include multiple regression programs that are capable of handling the analysis tasks described in this section.

For a given set of outcome measures, the principal task in specifying the regression equations to be estimated involves making decisions on which variables to include in the models. Because the objective of the analysis is to identify whether there are significant differences in the pre-program earnings and AFDC grants of the two groups after controlling for measured characteristics, one should adopt the strategy of including all potentially relevant factors, except those that are inappropriate for various statistical reasons. Specifically, we recommend that the following guidelines be used in making decisions concerning the independent variables to be included in the models:

1. Include all personal characteristics of the individuals at enrollment/registration that were examined as part of the analysis conducted for Criterion (1) discussed above (e.g., age, race, education, occupation, handicapped status), except those that must be omitted because too few cases exhibit that characteristic, or to serve as the "left-out category." For example, it is likely that in many states there will be too few American Indians/Alaskan Natives and Asians/Pacific Islanders (and in some states too few Hispanics) to include these as separate variables in the model. As a result, one may need to collapse the five racial/ethnic group variables into three variables: white dummy, black dummy, and other racial group dummy. It is also important to note that in estimating the regression model one of the race dummies must be omitted to serve as the "left-out category" or reference category for comparison purposes. For example, if the white dummy variable is omitted from the equation, then the coefficients of the other two race variables would represent the effect of being in that group relative to being white. It should be noted that for every set of dummy variables that are included in the regression model to capture the effects of a certain characteristic, one of the variables must be omitted to serve as the reference category for comparison purposes. Because the effects of the included variables are all measured relative to the left-out category, the results have the identical interpretation no matter which variable is chosen to serve as the left-out category.

2. Include all prior quarterly earnings and AFDC grants variables. That is, in examining the comparability of earnings in the immediate pre-program quarter, one should include quarterly earnings and AFDC grants from the second through the twelfth pre-program quarters, given data availability. If, however, one were examining the comparability of earnings and AFDC grants in the immediate pre-program year, then the second, third, and fourth pre-program quarterly earnings and AFDC grants variables would have to be omitted from the regression equation. This is, of course, because such variables are definitionally part of the dependent variable in this case and, as such, cannot independently affect its value.

3. Exclude variables that are "jointly determined" with pre-program earnings and AFDC. Specifically, the following three variables described in Chapter 4 should be excluded from all regression equations estimated over a pre-program period: AFDC recipient dummy, UI recipient dummy, and UI benefits in the immediate pre-program quarter.

4. One should also consider including potential "interaction" effects among various variables in the model. Interaction variables are simply the product of two (or more) variables

that are already entered separately in the model. They are included to allow the effect of a particular variable to vary depending on the value of another variable. For example, the effect of education on earnings may vary depending upon whether the person is white, black, or other; additional variables defined as the product of race dummies and education dummies would allow one to test for such a relationship. As discussed in the evaluation guide, because previous employment and training program evaluations often estimated entirely separate models by race, it will be particularly important to investigate whether the effect of other variables in the model (e.g., age, education, prior earnings) differs by race.

5. Finally, in selecting the variables to be included in the regression models, it is important that they be defined according to the appropriate time period. For example, if the dependent variable is earnings or AFDC grants in the immediate pre-program quarter (year) then the unemployment rate in the local area should similarly be defined as the rate for the immediate pre-program quarter (year).

By following the guidelines provided above, one could identify a set of independent variables from those created using the procedures described in Chapter 4 that should be included in both the pre-program earnings and AFDC grants equations. Based on these guidelines, below we provide an example of a set of independent variables that should be included in regression equations for adult men and women to examine the similarity of participants and comparison group members on earnings and AFDC grants in the immediate pre-program quarter:

- Age;
- Age squared;
- Black dummy;
- Other non-white dummy;
- High school graduate dummy;
- Post high school education dummy;
- (Age) x (white dummy);
- (Age squared) x (white dummy);
- (High school graduate dummy) x (white dummy);
- (Post high school education dummy) x (white dummy);
- Veteran dummy (for men only);

- Set of eight one-digit DOT dummies (e.g., allowing professionals to be the left-out category, the eight occupation dummies would correspond to clerical and sales; service; agricultural, fishery, and forestry; processing; machine trades; benchwork; structural work; and miscellaneous);

- Handicapped dummy (if measured comparably for both groups);

- Prior quarterly earnings (i.e., pre-program quarters 2 through 12, data permitting);

- Prior quarterly AFDC grants (i.e., pre-program quarters 2 through 12, data permitting);

- Unemployment rate during the immediate pre-program quarter;

- Urban location dummy;

- Set of dummies for the quarter of enrollment/registration (e.g., allowing the first quarter to serve as the left-out category, dummy variables for whether participants (comparison group members) enrolled (registered) in quarter 2, 3, or 4); and,

- JTPA participant dummy.

Given the above set of independent variables, the test for differences in earnings and AFDC grants between the participant and comparison groups in the immediate pre-program quarter would be based on a t-test of the estimated coefficient of the JTPA participant dummy variable. The t-ratio is given by the estimated regression coefficient divided by its standard error, and is provided as part of the printed output from all major software statistical packages. Given the large sample sizes involved, the hypothesis of no difference between the two groups in pre-program earnings or AFDC grants would be rejected at the .05 (.01) significance level if the calculated t-ratio exceeded 1.96 (2.58) in absolute value.[20] On a more intuitive level, because participation in JTPA during a given period can not have an effect on earnings or AFDC grants in previous time periods, the coefficient of the JTPA dummy variable in each of the regression models described above should be zero. The extent to which the estimated coefficients deviate from zero provides evidence on the direction and magnitude of the likely selection bias. That is, with earnings in the pre-program period as the dependent variable, large negative (positive) coefficients on the JTPA dummy would indicate that participants were less (more) advantaged than comparison group members in the pre-program period on unmeasured characteristics, and if this persisted through the post-program period it would likely result in understating (overstating) the net impact of JTPA on earnings. Thus, for example, if this analysis indicated that, after adjusting for differences in measured characteristics, the pre-program earnings of JTPA participants were $200 less (more) than the earnings of the comparison group, then one might consider adding (subtracting) $200 to (from) the net impact estimate to adjust for differences in unmeasured characteristics. It should be noted, however, that because pre-program earnings and AFDC grants will be included as independent variables in the net impact model, the extent of this bias should be less in the post-program period. As such, adjusting the net impact estimate for the total difference in pre-program earnings is likely to overcompensate for the bias due to differences in unmeasured characteristics.

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[20] One could also estimate a similar pre-program earnings or AFDC grants equation and replace the JTPA participation dummy variable with a set of variables representing different program activities to provide information on the differences in the pre-program outcome measures across program activities that are due to unmeasured characteristics. By testing whether these separate program activity coefficients are significantly different from zero, this would indicate whether there are likely to be any additional selection biases in the assignment of programs to individuals that will affect the net impact estimates by program activity that are described later in this section.

In analyzing the pre-program similarity of earnings and AFDC grants between the two groups, it is possible to examine several different time periods. For the most part, one should be primarily interested in examining the immediate pre-program quarter or year, and separate regression equations like the one described above could be estimated for both periods.[21] In addition, one could also estimate a regression equation like the one described above for each pre-program quarter and derive a set of estimated coefficients of the JTPA participant dummy. To the extent that including additional lagged values of earnings and AFDC grants in the equation serves to reduce the differences between the two groups, the coefficients of the JTPA dummy variable should be largest (in absolute value) in the early pre-program periods and tend toward zero as the pre-program outcome is measured closer to the date of enrollment.

The third criterion that we recommend be used to judge the adequacy of the comparison groups is the similarity of the relationships between earnings (and AFDC grants) and individual characteristics for JTPA participants and for comparison group members in the pre-program period. This criterion, which is considerably more strict than the previous two, is quite important because, if the same model is generating earnings (or AFDC grants) in the two groups, it suggests that program impacts will be less sensitive to other potential statistical problems. This would provide additional confidence in our ability to obtain unbiased estimates of program impacts.

To test for differences in the pre-program earnings (or AFDC grants) equations of participants and comparison group members, one would estimate a modified version of the regression equation that was described above to provide information on Criterion (2). Specifically, one would estimate an equation that included all of the explanatory variables listed above, plus each of the variables (except the JTPA participation dummy variable) multiplied by the JTPA participation dummy variable. The formal test of whether the earnings and AFDC grants equations in the pre-program period are different for participants and comparison group members is based on an F-test of the hypothesis that the coefficients of the interaction terms (i.e., the JTPA participant dummy multiplied by each of the other variables in the model) are all zero. This is is sometimes referred to as a "Chow" test. Most standard regression programs will allow one to perform an F-test of such an hypothesis and will also provide the calculated F-statistic for the test.[22]

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[21] As indicated in the second and fifth guidelines for selecting independent variables discussed above, the only changes necessary in the independent variables in changing the dependent variable from the immediate pre-program quarter to the immediate pre-program year would be to ensure that quarterly earnings and AFDC grants in the second, third, and fourth pre-program quarters were excluded and that the unemployment rate was defined for the entire pre-program year rather than just for the immediate pre-program quarter.

Under the assumption that the error terms are normally distributed, the test statistic follows Snedecor's F-distribution with r degrees of freedom in the numerator and N-K degrees of freedom in the denominator, where r is the number of restrictions being tested (i.e., the number of independent variables that have been multiplied by the JTPA dummy), and N-K is the number of degrees of freedom when no restrictions are imposed (i.e., total sample size less the number of variables in the equation). If the test statistic exceeded the critical value for the specified level of significance, then the null hypothesis would be rejected (i.e., we would conclude that the pre-program earnings (or AFDC grants) equations for the two groups are not similar). For example, suppose the pre-program earnings equation for adult men had 30 independent variables and a total sample size of 6,000. Then, there would be 30 degrees of freedom in the numerator and 5,940 degrees of freedom in the denominator, so the test statistic would follow an F(30, 5,940) distribution. Given the critical values for the F-distribution with these degrees of freedom, one would conclude that the earnings models of the two groups were different at a .05 (.01) significance level if the test statistic exceeded 1.46 (1.69).

The three criteria and related analyses described above should provide considerable information regarding the adequacy of the comparison groups in the pre-program period and the types of likely biases that must be dealt with. It should be emphasized again, that these criteria are relatively strict tests of the comparability of the two groups and one should not generally expect nonexperimentally-derived comparison groups to meet all of them. If the conditions are generally satisfied, then the chances of obtaining unbiased program net impact estimates using the standard statistical models described below are considerably improved. If the criteria are strongly rejected (e.g., F-statistics of 10 or 20 when approximately 1.5 is sufficient for rejection), then one should be very careful in proceeding to estimate net impacts with these comparison groups. Instead, one should first double check to ensure that the data processing and analysis guidelines described earlier were followed and, if the criteria are still strongly rejected, one should then consider obtaining assistance from a researcher familiar with these issues. If, as is most likely, the results are somewhere in between (i.e., pre-program differences between the two groups that are sometimes statistically significant, but not exceptionally large), then one will need to understand the implications of these differences for interpreting and adjusting the net impacts results.

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[22] If the regression packages available to a state do not have this feature, one should consult the evaluation guide where an alternative approach to performing this test is described.

## ESTIMATING THE AVERAGE NET IMPACTS OF JTPA PROGRAMS

We now turn to a description of the approach to be used to estimate the average net impacts of JTPA programs on participants' post-program outcomes. As described earlier, the four general post-program outcome measures are earnings, whether employed, AFDC grants, and whether an AFDC recipient. Below we discuss the specific post-program periods for which these outcomes will be measured for different samples of enrollees and describe the regression model to be used to estimate average net impacts. A subsequent section describes how to obtain separate estimates of net impacts for major demographic groups, by program activity, and by length of program participation.

### Choice of Post-program Periods and Implications for Potential Additional Sample Exclusions

As discussed in the evaluation guide, the choice of the post-program periods to be examined for samples of JTPA enrollees depends on the distribution of length of stay in JTPA. For example, if no individuals participated in JTPA longer than six months, then for a given quarterly sample of enrollees (e.g., those who enrolled during first quarter of PY 1985), all such individuals would have terminated from the program by the end of the third quarter of PY 1985. As such, earnings and AFDC grants received during the fourth quarter of PY 1985 would be the outcome measures for the first complete post-program quarter for these enrollees. If, however, there is considerable diversity in program length of stay and some individuals remain in the program much longer, one would have to decide whether to postpone the analysis and wait until all cases had terminated, or exclude such cases from the analysis samples. Although it is generally not desirable to restrict the participant sample to those who have terminated from JTPA by a particular date (because terminees could differ systematically from nonterminees which could result in additional selection biases), in most cases it will simply not be possible to wait for all participants to terminate from the program and still provide timely net impact results.

In order to provide timely results, we recommend that individual states choose a cut-off date that defines the in-program period, and that any participants who are in the program after that point be excluded from the analysis.[23] In general, we expect that defining the cut-off date of program participation to be six months after the end of the period of enrollment for each quarterly sample (e.g., March 31, 1986, for those who enrolled in JTPA during the first quarter of PY 1985) should be adequate to meet most states' needs. This allows for a program length of stay that is no less than six months for any individual and up to nine months for individuals who enrolled very early in a particular quarter. We expect that such cut-off dates, applied uniformly to participants in all four quarters of the program year, would result in excluding no more than ten percent of the participant sample in most states, which is unlikely to significantly bias the average net impacts of JTPA, and should also not significantly reduce the precision of the estimated impacts. For states that operate JTPA programs that tend to have very long program lengths of stay, they should consider extending the cut-off date, which will require an additional data collection effort and delayed results in order to estimate earnings impacts for the same number of post-program quarters. On the other hand, for states in which the JTPA services are relatively brief on average, it may be possible to define a cut-off date that allows for a shorter in-program period and, as a result, net impacts can be estimated over a longer program period without delaying the analysis. In the ideal situation, a cut-off date of six months after the end of the quarter of enrollment will result in excluding no (or only a minimal number of) participants from the analysis samples.

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[23] It should be noted that individuals who are still in the program in a given quarter should not be included when analyzing the impact of JTPA on earnings or AFDC grants during that quarter because their earnings may be unusually low (e.g., for classroom training or job search assistance participants) or unusually high (e.g., for OJT participants), which would bias the estimated overall net impacts.

As described in Chapter 2, we recommend that the net impacts of JTPA for adult men and women be estimated on each of the four general outcome measures for three different post-program calendar periods. Specifically, post-program net impacts are to be estimated for a three-month period, for a six-month period, and for a twelve-month period. Based on the data collection plan described in Chapter 4, and the strategy to be used to exclude long-term stayers (if necessary), the research design will enable one to estimate the net impacts of JTPA on these four measures over a three-month post-program period for JTPA enrollees from all four quarterly samples; it will enable one to provide net impact estimates on these outcomes measured over a six-month post-program period for the first three quarterly enrollment samples; and it will enable one to estimate the net impacts for a twelve-month post-program period only for participants who enrolled in the first quarter of the program year. Because of the different sample sizes involved in analyzing impacts for different post-program periods, the precision of the estimated annual net impacts will be much less than the precision of the estimated impacts over a three-month period. Because of the importance of longer-term impacts in making judgments concerning the effectiveness of employment and training programs, some states might consider expanding the research design to collect additional quarters of post-program information for individuals who enrolled in the last three quarters of the program year. It must be recognized, however, that the final analysis results would necessarily be delayed almost another year.

## Autoregressive Net Impact Models

As described in the evaluation guide, to estimate the net impacts of JTPA for adult men and women, we recommend that autoregressive models be used. Using this approach, ordinary least squares regression equations would be estimated for each of the twelve outcome variables, that is, four outcome measures in each of three different post-program periods. The underlying philosophy of the model is to control as much as possible for potential differences in the characteristics of participants and comparison group members. As such, the independent variables should include all pre-program characteristics that are comparably measured for participants and comparison group members. In particular, the approach derives its name (i.e., "autoregressive") because pre-program values of the outcome measures (i.e, quarterly earnings and AFDC grants) are also included as independent variables. Although this results in a rather inelegant net impact estimation equation, it has the primary advantage of controlling for any differences in measured characteristics between the two groups that remain after the matched comparison groups are selected, which helps to minimize the problem of selection bias.

To control for potential differences in the characteristics of participants and comparison group members to the extent possible, we recommend that the net impact regression model be a slightly expanded version of the models used to determine whether the comparison groups meet the pre-program comparability Criteria (2) and (3) above. The only changes in the independent variables to be included in the basic net impact model as compared to the versions of the models discussed above are as follows:

1. Quarterly earnings and AFDC grants in the immediate pre-program quarter should be included in the net impact model.

2. The net impact model should also include the UI recipient dummy variable, UI benefits earned in the immediate pre-program quarter, and the AFDC recipient dummy variable.

3. The unemployment rate should be defined according to the post-program period for which the model is being estimated.

Thus, following these guidelines, an example of an initial net impact model for earnings and AFDC grants for a three-month post-program period would include the following independent variables:

- Age;
- Age squared;
- Black dummy;
- Other non-white dummy;
- High school graduate dummy;
- Post high school education dummy;
- (Age) x (white dummy);
- (Age squared) x (white dummy);

- (High school graduate dummy) x (white dummy);
- (Post high school education dummy) x (white dummy);
- Veteran dummy (for men only);
- Set of eight one-digit DOT dummies (e.g., allowing professionals to be the left-out category, the eight occupation dummies would correspond to clerical and sales; service; agricultural, fishery, and forestry; processing; machine trades; benchwork; structural work; and miscellaneous);
- Handicapped dummy (if measured comparably for both groups);
- Pre-program quarterly earnings (i.e., pre-program quarters 1 through 12, data permitting);
- Pre-program quarterly AFDC grants (i.e., pre-program quarters 1 through 12, data permitting);
- AFDC recipient dummy;
- UI recipient dummy;
- UI benefits received in the immediate pre-program quarter;
- Unemployment rate during the post-program three-month period;
- Urban location dummy;
- Set of dummies for the quarter of enrollment/registration (e.g., allowing the first quarter to serve as the left-out category, dummy variables for whether participants (comparison group members) enrolled (registered) in quarters 2, 3, or 4); and,
- JTPA participant dummy.

With such an autoregressive model estimated separately for adult men and women, the estimated coefficient of the JTPA participant dummy variable represents the average net impact of JTPA on earnings and AFDC grants for a three-month post-program period for both groups. Similar autoregressive models would be estimated for the six-month and twelve-month post-program periods for adult men and women, and the estimated coefficient of the JTPA participant dummy variable in these models will also provide an estimate of the average net impact of JTPA on the post-program outcomes during these periods. For dependent variables that are expressed in dollar terms (i.e., earnings and AFDC grants), the coefficient of the JTPA participant dummy variable can be interpreted as the average dollar impact on a given outcome measure. By dividing the estimated dollar impact by the mean earnings or AFDC grants of comparison group members, this provides an estimate of the percentage change in earnings or AFDC grants due to JTPA.

For dummy dependent variables (i.e., whether employed in a particular period, or whether receiving AFDC grants during a particular period), the autoregressive net impact model is equivalent to a linear probability model. That is, the model essentially estimates the effects of various factors on the probability of a certain event occurring (e.g., having positive earnings in a given post-program period). As such, the estimated coefficient of the JTPA participant dummy variable can be interpreted as the average percentage point change in the probability of working or receiving AFDC grants due to JTPA. Once again, by dividing the estimated percentage point change by the mean proportion of comparison group members, one can obtain an estimate of the percentage change in the probability of working (or receiving AFDC) due to JTPA.

## OBTAINING NET IMPACT ESTIMATES FOR VARIOUS SUBGROUPS

The models described above have focused on providing overall estimates of the net impacts of JTPA for adult men and women. It is also of considerable policy importance to determine whether the effectiveness of JTPA varies by the type of program activity or by personal characteristics. Because these factors (i.e., JTPA program activities and participant characteristics) may change considerably over time, knowledge of how program net impacts vary among them would help interpret time trends in JTPA's impacts. Furthermore, information on which programs work best for which types of participants could provide valuable information for targeting future employment and training programs to the disadvantaged. Although the approach to estimating net impacts for different subgroups is formally identical, whether the subgroup refers to the type of program activity or to individual characteristics, additional selection biases are likely to arise in some situations. Below we describe how to modify the autoregressive earnings and AFDC grants models described above to estimate the net impacts of JTPA for various subgroups and indicate the additional biases that one must be aware of in interpreting the results.

In general, subgroup effects are estimated by including in the regression equation an interaction term that represents the product of the dummy variable for JTPA participation with the variable for the subgroup of interest. Suppose one is interested in testing whether the net impact varies by a characteristic that is represented by the three dummy variables $Z_1$, $Z_2$, and $Z_3$. For example, one might think of the three variables as representing race/ethnicity categories (white, black, other), or program services (CT, OJT, JSA).[24] Then, the only modification required to the autoregressive model described above involves replacing the JTPA participation dummy variable with three variables that each involve the JTPA dummy variable multiplied by one of the three variables representing the particular subgroup (i.e., JTPA x $Z_1$, JTPA x $Z_2$, JTPA x $Z_3$). The coefficients of these three variables are estimates of the net impact for the three subgroups of interest. To formally test whether the program net impacts differ significantly across the subgroups of interest, an F-test is used. In this case, the test statistic follows an F(r,N-K) distribution, where r is the number of restrictions imposed by the basic model (equal to the number of subgroups minus one), and N-K is the number of degrees of freedom in the basic impact model. The null hypothesis that the net impacts do not vary across the subgroups of interest (e.g., across racial groups) would be rejected for r=2 and sufficiently large sample sizes at the .05 (.01) significance level if the test statistic exceeded 2.99 (4.60). Most standard software analysis packages can calculate this F-statistic as part of the analysis run.

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[24] It should be noted that, in principle, similar analyses could be performed to determine whether net impacts vary across local labor market conditions. However, because the labor market variables would take on the same value for all persons in the same local area in a given time period, there is not likely to be sufficient variation in these variables to obtain precise estimates of how program impacts vary across local labor market conditions, except in large states, with many SDAs, and where there are considerable differences in labor market conditions across SDAs.

It should be noted that in attempting to disaggregate JTPA net impacts across subgroups, it is important that the subgroup characteristics also be included in the model as control variables to account for differences in the general level of earnings (or AFDC grants) across these subgroups. That is, in our illustration, the three $Z_i$ variables must also be in the model separately so that the estimated net impact coefficients only capture outcome differences due to JTPA across these subgroups and do not also capture the average differences in outcomes due to the $Z_i$'s themselves. In addition, it is also important that the subgroups be mutually exclusive and exhaustive. For example, suppose the $Z_i$'s refer to various participant age categories: 22-34, 35-54, and age 55-64. Then two types of problems can arise in estimating the net impacts for these age groups: (1) recoding errors can occur in creating the variables (e.g., ranges of 22-44, 35-54, 55-64) that result in overlapping the age ranges so that individuals age 35-44 would appear in both of the first two groups (i.e., subgroups are not mutually exclusive); and (2) individuals in the sample may not fall into any of the three age categories created (i.e., subgroups are not exhaustive). This could occur if some participants were younger than 22 or older than 64. If the subgroups are not exhaustive, then all of the participant observations that do not fall into one of the categories would be treated as comparison group members, which would result in biased estimates of the net impacts of JTPA for the other subgroups.

The interaction analyses described above will identify the types of individuals who benefit most from JTPA and whether there is a general pattern to the variation in program effectiveness. At a minimum, we recommend that states examine differential impacts by race, education, age, UI claimant status, AFDC recipient status, and pre-program earnings. Because individuals' pre-program characteristics cannot be affected by JTPA, no additional selectivity bias is introduced in disaggregating JTPA net impacts by demographic subgroups.[25] However, as we describe below, this may not be the case when examining whether JTPA effectiveness varies by program activity.

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[25] If the characteristics defining the subgroups of interest are not measured equally well for the participants and comparison group members, however, the subgroup impacts will inappropriately reflect these differences. That is, because the presence of measurement error

In principle, to probe beneath the average net impacts of JTPA and provide information on the program activities that contributed to the average effects, one would perform an identical interaction analysis to the one described above using $Z_1$, $Z_2$, $Z_3$, and $Z_4$ to represent classroom training, on-the-job-training, job search assistance, and other program assistance respectively. Then, if $c_1$ represented the estimated coefficient for the interaction term between the JTPA dummy and $Z_1$, then $c_1$ would be the estimate of the average net impact for CT, $c_2$ would represent the estimated net impact for OJT, $c_3$ would represent the estimated net impact for JSA, and $c_4$ would represent the estimated net impact for other JTPA activities. However, as we describe below, there is a major problem that threatens the internal validity of the by-program activity net impact analysis.

The problem is the familiar one of selection bias. In this context, it relates to the nonrandom assignment of program activities to JTPA participants. As described above, the assignment of program activity is likely to be based on the agency's perception of an individual's needs and abilities. To the extent that this assignment process is based solely on the measured characteristics of participants (i.e., age, race, sex, education, pre-program earnings), this will not bias the net impacts by program activity as these characteristics will be included in the net impact model. A much more serious problem arises if the assignment of program activities is based on unmeasured characteristics, such as motivation and ability, and those unmeasured characteristics also affect earnings. If, for example, the more-motivated participants are assigned to OJT programs and are also more likely to have higher earnings, then the estimated coefficient of OJT ($c_2$) may be large and positive. However, this significant coefficient of OJT on earnings would not measure the true effects of OJT on earnings, but merely reflect the fact that more-motivated individuals were assigned to the OJT activity. Thus, one must be very careful in interpreting net impacts by program activity because of this additional selection bias that can occur as a result of the assignment process.

As described in the evaluation guide, there are statistical procedures that can potentially be used to correct for such selection biases. These procedures rely on what are known as "instrumental variable" techniques. However, such techniques are only useful if one can identify variables to play the role of instruments. Specifically, one must find variables that affect the assignment of program activities to

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in an independent variable biases its estimated coefficient downward, if the amount of measurement error on a subgroup characteristic were greater in the JTPA sample, for example, than in the comparison group, the effect of that characteristic on the outcome variable would be smaller in the JTPA sample than in the ES registrant sample. The interaction term would inappropriately pick up such a difference and misleadingly indicate that JTPA impacts were smaller for individuals with that characteristic.

particular individuals but that do not affect participants' post-program outcomes. Although this is a difficult task in any circumstance, it will be particularly difficult given the limited number of individual characteristics available for the proposed analysis.

It may, however, be possible to identify potential instrumental variables from a carefully structured process analysis. As described in the SDA level A Guide for Process Evaluation (Volume III), an important aspect of the process analysis will be a detailed description of the process involved in assigning program activities to specific participants. Such an analysis might indicate, for example, that the assignment of program activities is influenced primarily by the availability of program-activity slots in a given SDA, and since the availability of program slots should not affect post-program earnings, such a variable could potentially serve as an instrument.

Provided one has identified one or more variables that can serve the role of instruments, the following steps must be followed to correct for selection bias using this procedure:

1. Estimate separate regression equations with dummy variables for each program activity as the dependent variable and include as independent variables all of the variables that appear in the basic net impact model (except the JTPA participant dummy) plus the instrumental variables. These equations should be estimated separately for adult men and women over the entire program year sample of participants.

2. Calculate a predicted probability for each participant of being assigned to each of the alternative program activities using the estimated coefficients from the regression models estimated in Step 1. Most analysis software packages have the capability of either producing these predictions as part of Step 1, or of saving the regression coefficients from the first step and allowing the analyst to prepare recode statements to compute the predicted values.

3. Replace the program activity dummy variables in the net impact equation with these predicted values for participants and set the values to zero for comparison group members. Provided the instrumental variables strongly affect the assignment of program activities to particular individuals but do not affect participants' post-program outcomes, the coefficients of the predicted program activities should produce consistent estimates of net impacts by program activity.

Even if the process analysis is not successful in identifying specific variables to play the role of instruments, it could shed considerable light on the validity of the program-activity net impact estimates and provide some indication of the likely direction of the selection biases. For example, the process analysis should be able to determine which program activities the more- and less-job ready participants are assigned.[26] Then, if the impact analysis finds that a given program activity has a large positive effect on post-program earnings (negative effect on AFDC grants) and the process analysis indicates that the more disadvantaged individuals are assigned to that activity, it would increase confidence that the observed relationship is causal and not due to selection bias. If, however, the process analysis indicates that the individuals assigned to that program activity are considerably more advantaged, then one should not be very confident that the net impact estimates for that program activity solely reflect the effects of the program.

## OBTAINING NET IMPACT ESTIMATES BY PROGRAM LENGTH OF STAY

In addition to providing evidence on how the net impact of JTPA varies by individual characteristics and program activities, it may be useful to examine whether the impact of JTPA varies by participants' program experiences. For example, several studies have attempted to determine how the impacts of employment and training programs vary with length of stay in the program. Although the results of such analyses could provide important information about the mechanisms through which employment and training programs produce their effects, as we describe below one must be careful in interpreting the results due to the familiar problem of potential selection bias.

To investigate whether the net impacts of JTPA vary by length of stay in the program, one would estimate an autoregressive model like those described above with the overall program participation dummy variable replaced by JTPA dummy variables interacted with variables developed to measure length of program participation.[27] For example, one could categorize the length of stay variable into four dummy variables corresponding to stays of less than one month, one to three months, three to six months, and greater than six months, and interact these four dummies with the JTPA participation dummy. Then the estimates of the four program interaction coefficients would represent the average impacts of JTPA for individuals with these different lengths of stay.

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[26] Information obtained from comparing the measured characteristics of participants across program activities should also provide some information on the probable direction of the selection bias. For more detail, see the discussion above of the analysis proposed to determine whether the comparison group meets comparability Criterion (1).

[27] One might also consider including a dummy variable that represents whether the participant completed the program activity the person was assigned to, as well as an interaction term between length of stay and completion status. Such variables will help to control for biases due to measurement error. For example, it may help to account for problems that arise by combining enrollees in a student-learning-paced adult basic education class that has an extremely variable length of participation with individuals in an electronics classroom training program that may have a fixed period of training, and representing the JTPA treatment provided by a common classroom training dummy variable.

Alternatively, if the effects of length of stay on the outcomes are approximately linear, a convenient specification involves a model with a JTPA participation dummy and the participation dummy interacted with actual weeks in the program minus average number of weeks in the program. In this specification, the coefficient of the JTPA dummy represents the estimated impact of JTPA at the average length of stay, and the coefficient of the interaction term is an estimate of the dollar impact of an additional week of program participation.

Although it is straightforward to construct the length of stay variables and estimate the coefficients of the interaction terms in the net impact models, one must be very careful in interpreting the results because of the familiar problem of selection bias. Although the autoregressive earnings model controls for differences in measured characteristics between short- and long-term participants, it is likely that some differences in unmeasured characteristics remain. Individuals who leave the program early may be less motivated or, alternatively, may have found employment on their own. On the other hand, individuals who stay in the program a long time may do so because they have fewer other employment opportunities. Length of stay is also likely to depend on the type of program activity and SDA characteristics.

Because of these additional selection bias problems, one should use extreme caution in interpreting the estimated impacts by length of stay as representing causal relationships. In order to overcome these biases and obtain estimates of the net impacts of JTPA by length of stay that one has confidence in, it is necessary to statistically adjust for the selection process. One method would involve using instrumental variable techniques. Operationally, as described above, one would first estimate a regression equation to obtain a predicted value of length of stay. Such equations would be estimated using the participant sample only, and would include as independent variables all variables in the net impact model plus the instrumental variables. The predicted values would then be entered in the net impact equation for participants, and zeros would be included for comparison group members.

The success of such an instrumental variable procedure relies heavily on the ability to identify variables to play the role of instruments. That is, one must find variables that are highly correlated with length of stay, but that do not influence earnings or AFDC grants. Depending on the types of information obtained from participants and included in the JTPA MIS in different states, it may be possible to identify variables than can play the role of instruments for length of stay. For example, it may be possible to develop instruments from answers to very simple questions about participants' general satisfaction with JTPA services and whether participants were assigned to the type of program they wanted. That is, it seems plausible to assume that individuals who are satisfied with the program, or who are assigned to the program type they were most interested in, would remain in JTPA longer. At the same time, however, there is no obvious reason why satisfaction with the program or assignment to the desired training program should affect earnings or AFDC grants, independent of the effect of the training and the actual length of stay. Thus, these variables could be entered into a regression equation along with other demographic characteristics to explain program length of stay and then a predicted value for length of stay could be constructed and entered in the net impact equation in place of the actual length of stay.

For states that are very interested in overcoming these selection bias problems, they should carefully review the data in their JTPA MIS, as well as the information obtained during the process analysis, to see if variables that affect length of stay, but not earnings, can be identified. If successful, they should implement the instrumental variable approach and determine whether the variables selected in fact strongly affected length of stay. If not, then it is not necessary to go to the second stage of including the predicted value in the net impact model, as the predicted variable will be too highly correlated with other characteristics in the model, and the results will not be reliable. If, however, the instruments do strongly affect program length of stay, then the net impact model should be estimated with predicted length of stay replacing the actual value. If the instruments have been successful, then they will purge the correlation between the error term and length of stay, and result in consistent net impact estimates.

## ADJUSTMENTS FOR POTENTIAL DATA AND DESIGN DEFICIENCIES

The models described above involve a comparison of the UI Wage Records and AFDC Grants Records of JTPA participants with those of comparison groups derived from ES registrants. In addition to the problem of potential selection bias, there are some deficiencies in the UI earnings data and in the JTPA and ES data that may affect the results. For example, UI earnings are inadequate in that they do not reflect earnings from jobs that are not in covered employment, or earnings from jobs that are located across the border in other states. The JTPA and ES data are deficient because there is inadequate information on whether ES registrants participated in JTPA, which may result in a "contaminated" comparison group. In this section, we briefly discuss the likely extent to which the basic impact estimates will be affected by these data and design deficiencies and the types of adjustments that may be necessary.

In Chapter 4, we outlined procedures that could be used to reliably exclude individuals from the comparison group who were currently participating in JTPA, who had participated in JTPA in the previous year, or who participated in JTPA during the post-program periods being examined. If, however, for various reasons it is not possible to implement these procedures, the comparison group may be "contaminated" to a certain extent. Such contamination would lead to an underestimate of the net impacts of JTPA, since it would effectively dilute the treatment, as some comparison group members would have also received JTPA services. We do not believe, however, that the net impact estimates will be seriously affected by this potential contamination problem, as the magnitude of the problem is likely to be smaller than one might expect. That is, although the ES is one source of applicants for JTPA programs and, as such, one might expect that contamination could be high, existing data indicate otherwise. For example, based on data for the State of Washington for PY 1985, only 0.1% of all ES registrants active during the year were recorded as having enrolled in JTPA programs. Moreover, only 0.3% of those economically disadvantaged enrolled in JTPA. Although the figures are somewhat higher for enrollment in any training program (e.g., JTPA, Job Corps, WIN, other) --1.0% for all applicants and 3.1% for those economically disadvantaged --even these participation rates are small enough to be safely ignored.

In states that have higher probabilities of economically disadvantaged ES registrants enrolling in JTPA, and in which it is not possible to exclude those who participate in JTPA from the comparison group before conducting the net impact analysis, it may be necessary to make some aggregate adjustment to the net impact estimates. Specifically, if p (q) is an estimate of the proportion of the adult men (women) in the comparison group participating in JTPA during the period of enrollment through the post-program period (i.e., from one to two years), the estimated average program net impacts for adult men (women) should be multiplied by $1/(1-p)$ $(1/(1-q))$ to adjust for this problem. Although it is difficult to obtain precise estimates of p and q, we believe that a reasonable approach would use values in the range of .05 to .10.

The second major data deficiency concerns the fact that UI Wage Records do not include jobs in nonreporting employment, nor do they include earnings from jobs in other states. As discussed in the evaluation guide, because the net impacts are based on differences in earnings between participants and comparison group members, the omission of earnings due to these problems biases the estimated impacts of JTPA only if program participation affects the probability of working in uncovered employment or the likelihood of working in another state. Although this was an important concern in evaluating the net impact of CETA programs given the high likelihood of CETA PSE participants turning their training slots into subsequent jobs in the public sector (which are less likely to be in reported employment), given the focus of JTPA on employment in the private sector, this should be less of a problem for the state-level net impact model. Moreover, in order to create a meaningful adjustment one would need information on interview-reported earnings and UI earnings for both groups in the post-program period, which will not generally be available. Thus, the best one can do is acknowledge the potential problem and indicate that the net impact estimates are based on the reasonable assumption that JTPA does not affect the probability of working in nonreported employment or working across the border in other states.

Finally, the most important potential adjustment to be considered is for selection bias, that is, systematic differences between participants and comparison group members that cannot be directly controlled for in the autoregressive net impact model. One can attempt to adjust for selection bias in two ways: (1) use evidence on pre-program differences between the two groups, or (2) estimate other more complex statistical models that under certain assumptions can correct for differences in unmeasured characteristics between the two groups. Using the first approach, one would use estimates of differences in adjusted pre-program earnings or AFDC grants between participants and comparison group members that are due to unmeasured characteristics that are developed as part of the analysis to examine the adequacy of the comparison groups described earlier in this section. The size of the estimated difference in pre-program earnings due to unmeasured characteristics is a reasonable estimate of the amount by which earnings impacts could be overstated or understated (depending on whether it is positive or negative) if the difference persisted in the post-program period. Thus, for example, if adult men (women) JTPA participants are estimated to have earned $100 more ($200 less) in the immediate pre-program year than individuals in the comparison groups, using this first approach one would adjust the main impact estimate (i.e., the coefficient of the JTPA dummy variable) downward (upward) by $100 ($200) for men (women). It should be noted, however, that because pre-program earnings are controlled for in the autoregressive net impact model, this adjustment may overcompensate for the selection bias due to differences in unmeasured characteristics between the two groups.

The second approach to correcting for selection bias involves using somewhat more complex statistical models to estimate the net impacts of JTPA. For example, as described in the evaluation guide, "symmetric-difference" regression models can overcome problems due to differences in unmeasured characteristics between the groups and yield unbiased net impact estimates provided certain assumptions are met. It should be noted, however, that such models require considerably greater longitudinal data--at least three to five years of UI Wage Records and AFDC Grants Records--which will limit their usefulness in certain states. States that are interested in estimating such models to correct for selection bias should consult the evaluation guide.

# CHAPTER 6

# COST ANALYSIS AND BENEFIT-COST COMPARISONS

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**Benefit-Cost Framework**
Benefits
Costs
**Estimating Program Costs**
**Benefit-Cost Comparisons**

## CHAPTER 6.

## COST ANALYSIS AND BENEFIT-COST COMPARISONS

The previous chapters of this implementation guide have described how to estimate the net impacts of JTPA on participants' post-program labor market experiences. Results of these analyses could be used to estimate the benefits of JTPA to participants and, under certain assumptions, to estimate the benefits of JTPA to society as a whole. In order to determine whether JTPA is an efficient use of public resources, however, it is necessary to assess the costs of providing JTPA services, and compare the costs and benefits of the program. Moreover, to make informed decisions about the design of the program, policy-makers must know both the costs and benefits of specific program activities. That is, program activities that yield relatively small benefits may yet be very effective when compared to the costs involved. In this chapter we provide a general framework for comparing the costs and benefits of JTPA and describe specific procedures for calculating the benefits and costs of JTPA.

The purpose of a benefit-cost analysis is to judge the social efficiency of a program. As such, it attempts to determine whether the value of the goods and services available to society is greater as a result of the program or whether the value would have been even greater had the resources been put to alternative uses. In order to make this determination, all of the benefits of the program are assigned a monetary value, and the present value of the benefits is compared to the present value of the program costs. Assuming that all present and future benefits and costs are identified, that appropriate monetary values are assigned, and that an appropriate interest rate is used to discount future benefits and costs into current values, then JTPA would be judged to be a worthwhile use of public resources if the present value of the benefits is larger than the present value of the costs.

Although the process involved in conducting a benefit-cost analysis is conceptually straightforward, there is, in practice, a variety of issues that limit the validity of any benefit-cost analysis. As a result, because of data limitations and other issues described in detail in the evaluation guide, it will not be possible for states to conduct a comprehensive benefit-cost analysis to provide a definitive estimate of the return on the investment to JTPA. Nevertheless, the general approach described below should be a useful tool to states in organizing information on program benefits and costs and should enable them to get an overall sense of the effectiveness of JTPA and the conditions under which JTPA can be regarded as an efficient use of public resources.

The discussion below begins with a brief description of a benefit-cost framework for analyzing the effectiveness of JTPA. We then briefly describe how to use the net impact estimates to measure some of the important social benefits due to JTPA. We then describe how to conduct a cost analysis to estimate the marginal cost of serving additional JTPA participants and the marginal costs of different program activities. Finally, we discuss the issue of discounting future benefits and costs so that the benefit-cost comparisons can be made in constant dollar terms and describe a few additional comparisons that should be made to determine how sensitive the overall conclusions are to certain assumptions.

## BENEFIT-COST FRAMEWORK

The benefit-cost framework presented in Exhibits 5 and 6, respectively, lists the major benefits and costs that would ideally be accounted for in conducting a comprehensive benefit-cost analysis of JTPA. As an aide to keeping track of the different benefits and costs, they are presented from three perspectives: the participant, the taxpayer, and society as a whole. The first class of benefits and costs consists of those that are received by or borne by program participants. The participant perspective is important because it sheds light on individuals' incentives to participate in the program. The taxpayer perspective, sometimes referred to as the nonparticipant perspective, is important because it reflects the effects of the program on the government budget. Finally, the most inclusive set of program benefits and costs are those accruing to society as a whole. These are simply the sum of benefits and costs received or borne by participants and taxpayers, taken separately. As such, the social perspective ignores transfers between segments of society (i.e., participants and taxpayers) and examines whether the program results in a net increase in the resources available to society.[28] This is the appropriate perspective for examining the overall effectiveness of JTPA.

### Benefits

As indicated in Exhibit 5, the major benefit of JTPA from the social perspective is the increase in output produced by participants. Conceptually, two types of output gains should be distinguished: (1) the increase in post-program output, and (2) the increase in output produced while participating in the program. For the most part, the in-program output due to JTPA is likely to be small, particularly for participants in classroom training and job search assistance program activities. Only for OJT programs is the value of in-program output likely to be substantial, and even for these programs, it is difficult to assign appropriate monetary values without an extensive data collection and analysis effort. Because of the difficulties involved in measuring the value of in-program output, as well as in measuring the value of other nonmonetary benefits (e.g., reduced crime), we recommend that states and SDAs do not attempt to directly measure these benefits, but recognize their potential importance when discussing the overall results from the benefit-cost analysis. As a result, the primary benefit to be measured is the increased post-program output due to JTPA.

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[28] Reductions in transfer payments (e.g., AFDC grants) do not represent a benefit from the social perspective since the increased benefit to taxpayers is offset by the loss of income to recipients, and there is therefore no change in the resources available to society as a whole. Thus, including estimated benefits from reductions in welfare dependency due to JTPA would involve a double counting of benefits.

As we describe in the evaluation guide, to measure the increase in output of goods and services available to society in the post-program period due to JTPA, we recommend that the participant-comparison group differences in earnings in the post-program period be used. This is a reasonable procedure provided that JTPA participants do not find jobs in the post-program period at the expense of other disadvantaged persons (i.e., program participants do not displace other job seekers).[29] It is beyond the scope of the state-level model to assess the extent to which individuals who participated in JTPA displaced other job seekers. As a result, the benefit-cost analysis is limited to determining whether the benefits to participants from receiving JTPA services are greater than the costs to society of providing those services.

In translating participant earnings gains into a measure of the increase in output of goods and services available to society, two additional issues arise. First, one must determine how to extrapolate the post-program gains observed for the periods from three months to one year following termination into subsequent periods. For example, if the three-month, six-month, and one-year earnings impacts imply similar gains per quarter, then it may be reasonable to assume that the gains persist over time. However, based on previous studies, it is likely that the gains decline over time, and information through just the first post program year may not be sufficient to estimate reliable time trends for the purpose of extrapolating future gains. Because of this ambiguity, the benefit-cost analysis should indicate whether earnings gains observed during the one-year follow-up period are sufficient to make the program worthwhile, as well as indicate the assumptions about future benefits that are necessary to make JTPA viewed as a worthwhile investment.

A second issue concerns adjustments that should be made to earnings gains to account for fringe benefits. That is, if the increase in output is equal to the increase in compensation paid to those who participate in JTPA, then although this compensation is primarily in the form of monetary earnings, adjustments for nonmonetary earnings should also be made. Fringe benefits include pensions, health and other forms of insurance, and payments on behalf of the worker for unemployment insurance and worker compensation, and FICA. As a rough approximation, insurance and pension benefits for workers served by JTPA are estimated to be approximately 10% of monetary benefits, and payments to government programs are approximately 10% as well. Thus, we recommend that the net earnings gains be multiplied by 1.2 to adjust for fringe benefits in deriving a measure of the social benefits due to JTPA.

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[29] Although very unlikely, in the extreme the program could produce no net increase in output despite large increases in participants' post-program earnings by simply reshuffling jobs from nonparticipants to participants.

To summarize, the benefits to be measured and included in the benefit-cost analysis include only the increase in post-program output due to JTPA. This will be approximated by the increase in post-program earnings, as measured by the estimated coefficient of the JTPA dummy variable in the net impact equation (and potentially adjusted for data and research design limitations as described in Chapter 5), adjusted for fringe benefits. Procedures will be developed to determine how the increase in earnings over the first year should be extrapolated to yield estimates of increases in post-program output in subsequent years. These steps will yield an estimated stream of future benefits for both adult men and women. Individual values in these benefit streams will then be weighted by the proportion of men and women served by JTPA in the particular program year to generate an estimate of the aggregate benefit stream due to JTPA.

## Costs

As indicated in Exhibit 5, there are several different cost components in a benefit-cost analysis of an employment and training program. The major cost categories include (1) program operating costs; (2) participant opportunity costs; (3) transfers to participants; (4) costs associated with participating in the program activities; (5) psychic costs to participants of participating in JTPA; and (6) stipends and other income maintenance transfers such as UI benefits received during training. Each of these cost categories is discussed briefly below.

The major costs from the social perspective are the program operating costs. The costs of operating employment and training programs include direct operating costs such as premise rent, salaries for instructors, and costs of materials and supplies, and indirect costs such as the costs of managing and administering the program. As indicated in Exhibit 5, because program participation is free, the operating costs are not considered as costs from the perspective of program participants. However, operating and administrative costs do involve the use of considerable resources that have alternative uses and, as such, they represent real costs from the perspective of the taxpayer and society as a whole. Thus, in Exhibit 5, these costs are represented as a zero to program participants and as a minus in the other two columns.

A second important component of cost concerns the earnings opportunities that participants may forego while participating in the program. These foregone earnings are clearly costs to participants and, to the extent that less output is produced because workers were participating in JTPA, the foregone output (as measured by foregone earnings) is a net cost to society as well. Although previous studies have recognized foregone earnings to be an important element of program cost, because of data limitations and certain evaluation issues this cost component is almost always excluded from the final benefit-cost

Exhibit 5

## A GENERAL TAXONOMY OF THE BENEFITS AND COSTS OF JTPA FROM DIFFERENT PERSPECTIVES

| | Perspective | | |
|---|---|---|---|
| | Participant | Taxpayer | Social |
| **Benefits** | | | |
| • Increased output | | | |
| - Post-program output | + | 0 | + |
| - In-program output | 0 | + | + |
| • Reduced receipt of transfers | | | |
| - Reduced welfare recipiency | | + | 0 |
| - Increased taxes | - | + | 0 |
| • Nonmonetary benefits | | | |
| - Reduced crime | - | + | + |
| - Reduced use of alternative social programs | 0 | + | + |
| - Improved work attitudes of participants | + | 0 | + |
| **Costs** | | | |
| • Program operating costs (e.g., premise costs, staff costs, materials and supplies, and administrative costs) | 0 | - | |
| • Participant opportunity costs (e.g., foregone earnings) | - | 0 | |
| • Transfers to participants (e.g., stipends) | + | - | 0 |
| • Costs of participation (e.g., transportation, child care) | - | 0 | - |
| • Psychic costs | - | 0 | - |

comparisons. Although the social program costs may be somewhat understated by this exclusion, given the relatively brief length of participation in JTPA and the questionable nature of the assumption that considerable output was foregone when previously unemployed workers participated in the program, we recommend that this cost component be formally omitted from the benefit-cost analysis. However, in interpreting the results of the benefit-cost comparisons, one should indicate to what extent the overall assessment is likely to be sensitive to omitting the social cost of foregone earnings.[30]

Other potentially important costs of JTPA from the taxpayers' perspective are the transfers to participants in the form of stipends and other income transfer payments, and transportation and childcare allowances. Although such costs are much less important under JTPA than under CETA, they could be considerable in some cases. It should be noted, however, that such transfers are a cost from the taxpayers' perspective, and a benefit to the participants that receive them and, as such, they do not affect the social cost of the program.

Other potentially important costs concern the direct cost participants incur in participating in JTPA activities as well as any psychic costs. The psychic costs are inherently unmeasurable and are included in the conceptual framework only for the purpose of completeness. Moreover, although the costs incurred by participants in attending classes or participating in job search activities that they would not have done if they had not participated (e.g., increased transportation, childcare, special clothing/equipment) are potentially measurable, they require data from individual participants. Because of the large expense involved in acquiring such information, and given that these costs are likely to be a small share of the total cost of the program to the individual and to society as a whole, as a practical matter these costs will also be omitted from the final benefit-cost comparisons.

Thus, to summarize, the costs of JTPA to be measured and included in the benefit-cost analysis will be limited to the costs involved in operating the program (i.e., the sum of premise rent costs, staff costs, materials and supplies, and administrative costs). In interpreting the benefit-cost analysis comparisons, however, it will be important to recognize that many of the social costs of JTPA have not been measured and that these unmeasured costs could affect the overall assessment of whether JTPA is an efficient use of social resources. Below we describe an approach to estimating the operating costs of JTPA.

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[30] For example, one could obtain a gross estimate of the foregone earnings from data on the earnings of comparison group members during the program period. Specifically, one could use the earnings of comparison group members for the quarter of enrollment plus half of their earnings for the subsequent quarter to approximate foregone earnings over a four-and-a-half month period, roughly the average length of stay in JTPA. In interpreting the final benefit-cost results, one could then indicate whether adjusting for this gross estimate of foregone earnings affects the overall conclusions.

## ESTIMATING PROGRAM COSTS

Because the benefits from JTPA will be expressed in terms of dollars per individual adult participant, the cost analysis must similarly estimate the incremental (i.e., marginal) cost in dollar terms per individual adult participant. As described in the evaluation guide, although there are many obstacles to deriving reliable estimates of the marginal costs of serving JTPA participants (e.g., problems of data omission and inconsistent aggregation, difficulties in allocating input costs among joint outputs, ambiguity involved in imputing prices of owned or subsidized inputs), statistical methods are available that can overcome several of these problems and provide useful information on the marginal costs of employment and training programs. Below we describe one method that states could use to estimate the marginal cost of serving adults in JTPA. States that are interested in alternative methods or ways of estimating program costs at the SDA level should consult the evaluation guide.

The primary source of data for the cost analysis will be the JTPA Annual Status Report (JASR). The JASR provides for each SDA the characteristics of program terminees, and information on program outcomes and costs, for Title II-A and Title III programs funded under JTPA.[31] Fortunately, for our purposes, these data are provided separately for adult and youth participants in Title II-A programs. Moreover, because these data are structured for reporting to the federal government, they are consistently available for all SDAs and the quality of the information appears to be very good.

As described in the evaluation guide, these data have several advantages for estimating the marginal cost of serving JTPA participants. In addition to being in a standardized format with unambiguous definitions of all information items, the JASR contains information on total federal expenditures in operating JTPA (although not total social costs because it includes stipends and excludes opportunity costs and other costs of program participation), as well as considerable information on the socioeconomic characteristics of adult program terminees (e.g., number of terminees by sex, age, education, racial/ethnic group, welfare recipient status, limited English language proficiency, handicapped status, and average number of weeks participated). These participant characteristics can be thought of as inputs that enter the employment and training production process and that have obvious instructional and resource implications that will affect costs (e.g., those participants with limited English language proficiency will likely require more program resources to complete training). As such, these characteristics can be used to standardize the relationship between total costs and participants served to obtain estimates of marginal costs as described below.

Although the JASR contains considerable information and could serve as the basis of a cost analysis, it also contains two major limitations.

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[31] Although the cost information corresponds to program terminees and the benefit information is based on net impact estimates for samples of program enrollees, this should not cause any comparability problems as long as program costs do not vary widely from year to year.

Specifically, the JASR does not contain (1) data on the number of terminees by type of program activity or (2) information on the costs incurred at the state level in operating JTPA programs. Without information on the number of individuals served by type of program activity, it is not possible to isolate how the marginal costs of JTPA activities differ, which would in turn preclude one from comparing the marginal benefits and marginal costs of different program activities to identify those activities that are most effective. In addition, without information on the costs incurred at the state level, the marginal costs of serving JTPA participants as derived only from JASR data would be understated.

In order to overcome these problems, we offer two recommendations. First, and most important, every effort should be made to obtain data on the number of adult terminees that participated in various program activities during the program year. At a minimum, it would be useful to have information on the number of classroom-training-only terminees, the number of OJT-only terminees, the number of JSA-only terminees, the number of terminees who only participated in some other activity, and the number of terminees who participated in multiple activities. In order to be useful for the cost analysis, this information would have to be obtained from each SDA, and for the same period in which the terminee characteristics and program costs on the JASR are reported. This could be accomplished either by adding the data elements to the JASR form and requesting SDAs to provide the information, or by developing the necessary computer software to extract the information from the JTPA MIS. For example, one could implement the following steps to obtain the necessary information for PY 1985.

1. Create a working file (on tape) of all persons who terminated from JTPA Title II-A programs in any SDA in the state during PY 1985. The file should include the person's age, data on all program activities participated in, and an SDA identifier.

2. Exclude from the file all persons who are 21 or younger.

3. Create variables that represent each type of treatment of interest, and that may have different cost structures. For example, as indicated above, it is likely to be important to at least differentiate the costs by type of activity as well as costs for those who participate in only one activity versus multiple activities. This can be accomplished by creating five variables, the first four of which would simply be dummy variables indicating whether the only activity the person participated in was CT, OJT, JSA, or other, and a fifth variable indicating whether the person participated in any combination of these activities.

4. Stratify the adult terminee file by SDA. That is, create a separate subfile of adult terminees for each SDA.

5. Create counts of the number of individuals in each SDA in each of the five program treatment types and merge these counts with the JASR data for each SDA.

Our second recommendation concerns how to handle costs incurred at the state level in the operation of JTPA programs. Conceptually, the costs (actual or imputed) incurred at the state level in the operation of JTPA programs should be added to annual program year SDA total costs to obtain a better estimate of the total social costs of JTPA. Provided information is available on the total costs contributed by the state to the operation of JTPA, it would be possible to apportion these costs to the various SDAs. For example, one method would be to assume that costs incurred at the state level in support of various SDAs are proportional to the number of adult terminees in the SDA. Thus, to allocate state-level costs in operating JTPA programs to the different SDAs, one could multiply total state costs by the ratio of the number of adult terminees in a given SDA to the total number of adult terminees in all SDAs. Such a procedure would, in part, overcome the limitation of the JASR data described above. If, however, it is not possible to obtain estimates of costs contributed at the state level to the operation of local JTPA programs, this limitation would also have to be recognized in interpreting the results of the benefit-cost analysis.

With the basic data set and adjustments described above, it is possible to estimate a program cost function that can provide information on the marginal cost of serving JTPA participants for the benefit-cost analysis. For example, using ordinary least squares regression techniques, one could estimate a regression equation with total federal expenditures plus allocated costs incurred at the state level (if possible) for the SDA as the dependent variable, and independent variables would include:

- Number of adult terminees;
- Number of adult terminees who are:
    - Male;
    - Black;
    - Hispanic;
    - Other nonwhite;
    - Students (high school or less);
    - High school graduates;
    - Age 22-54;
    - Welfare recipients;
    - Single household heads with dependent children;
    - UI claimants;
    - Limited English language proficiency;
    - Handicapped; and,
- Average number of weeks of participation.

With these independent variables in the regression equation, the coefficient of the variable "number of adult terminees" would represent the marginal cost of serving additional adult participants in JTPA on average, and the coefficients of the other variables in the model would capture the extent to which the marginal cost varied for serving persons with specific characteristics.

If the procedures outlined above are followed so that data on the numbers of terminees by program activity are obtained for each SDA, then one would estimate a second regression equation like the one above except that the "number of adult terminees" variable would be replaced by the following four variables: the number of classroom-training-only terminees, the number of OJT-only terminees, the number of job-search-assistance-only terminees, and the number of terminees that participate in multiple activities. In this formulation, the coefficients of these four variables would represent estimates of the marginal cost for each of the different types of program activities (CT only, OJT only, JSA only, or multiple activities). These estimates of the marginal costs of serving adult JTPA participants (either overall or by program activity) would be compared to the marginal benefits from the program in terms of increased post-program output (either overall or by program activity) to make statements about whether JTPA is an effective use of public resources as described below.

Before discussing the benefit-cost comparisons to be made, one additional potential limitation of the cost analysis should be noted. This concerns the issue of sample size. That is, since the analysis is based on SDA-level data, the number of observations available in an annual cross sectional analysis equals the number of SDAs in the state. In relatively small states with few SDAs, there may be insufficient degrees of freedom to estimate a cost equation like the one given above and obtain reliable estimates of program marginal costs. In such cases one should consider using quarterly JASR data on each SDA (and including dummy variables for different quarters to account for seasonal cost differences and costs that may occur at the end of the contract year), or other less formal methods that are described in the evaluation guide.

## BENEFIT-COST COMPARISONS

In conducting a benefit-cost analysis, three data elements are required: estimates of the benefit stream over time, estimates of program costs over time, and the interest rate used to discount future benefits and costs into current dollars. In the discussion above we have described how to develop monetary measures of the benefits and costs of JTPA. Below we briefly indicate how to discount the future benefit stream so that the present value of benefits can be compared to the current program costs and indicate the criteria to be used to measure the net effectiveness of JTPA as a social investment.[32] The chapter concludes with some examples of other comparisons that should be made to determine how sensitive the overall conclusions are to alternative assumptions.

As discussed in the evaluation guide, because the benefits of employment and training programs occur over time, it is important to translate this stream into a common reference period. Conventionally, this has involved discounting future dollars into present terms, using an interest rate that approximates the alternative costs of the funds invested.

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[32] It should be noted that in most benefit-cost analyses, costs are also incurred in future periods and require discounting as well. However, given the short-term nature of the JTPA program, all costs are incurred in the current period so that no discounting is necessary.

Choice of an interest rate to be used in discounting is not as easy as it would seem at first blush. There are competing theories as to how to select the correct interest rate. It is not necessary to discuss these here except to note that the rate will differ for society as a whole compared to, say, a state government or an SDA. The rate for society as a whole is net of any effect due to inflation, while this is not the case for a local government or agency. The most practical guidance on the choice of interest rate is to select that rate at which the local government or agency can lend its funds. The accounting office of a local government or agency can provide that figure. Currently, lending rates are in the neighborhood of 10%, which is a relatively high number.

Using a 10% discount rate, one would measure the net effectiveness of JTPA by calculating a benefit-cost ratio, where the numerator is the present value of the incremental benefits due to the program (i.e., $\sum_{t=1}^{n} B(t)/(1.10)^t$), the denominator is the present value of the costs incurred (which require no discounting since all costs are incurred in the current period), "t" refers to the post-program years in which benefits are realized, and "n" is the last year in which benefits are realized. Within the numerous limitations described earlier, JTPA would be regarded as an efficient use of public resources whenever the benefit-cost ratio exceeded 1. Provided one was able to derive estimates of the benefits and costs of each of the various program activities, similar calculations could be made to determine which program activities had larger benefit-cost ratios and could be considered to be more cost-effective.

In addition to obtaining the main benefit-cost results described above, which are based on the "best" estimates of the benefit stream, current program operating costs, and a 10% discount rate, we believe it is important that benefit-cost ratios be calculated to demonstrate how sensitive the conclusions are to alternative assumptions. In particular, alternative benefit-cost ratios should be calculated for.

1. One additional discount rate, say 3%.

2. Benefit estimates that do not include adjustments for selection bias or for potential contamination. Since each set of estimates rests on a different set of assumptions that are inherently untestable, it is important to know how sensitive the overall conclusions are to the size of these adjustments.

3. A range of program benefits and costs that reflect the fact that the main estimates are subject to statistical imprecision. For example, one could construct an upper and lower bound of a 95% confidence interval for the net impact of JTPA on post program earnings by adding and subtracting 1.96 multiplied by the

standard error of the JTPA dummy variable to the estimated JTPA coefficient. By adjusting both the upper and lower bounds for the fringe benefits, one would then obtain an estimate of the upper and lower bounds for the increase in post-program output due to JTPA. Upper and lower bounds for the marginal cost of JTPA can also be obtained by creating a 95% confidence interval around the appropriate regression coefficient (i.e., adding and subtracting 1.96 multiplied by the standard error of the estimated coefficient of the number of adult terminees in the cost equation). Then, by choosing different combinations (e.g., upper bound for benefits and lower bound for costs, lower bound for benefits and upper bound for costs), one can provide useful information on how sensitive the benefit-cost ratios are to alternative assumptions.

These sensitivity analyses, in combination with the main benefit-cost results, should provide a wealth of information on the effectiveness of JTPA programs.

# CHAPTER 7

# RELATIONSHIP AMONG MODELS: NET IMPACT, GROSS IMPACT, AND PROCESS ANALYSIS MODELS

# CHAPTER 7. RELATIONSHIP AMONG MODELS: NET IMPACT, GROSS IMPACT, AND PROCESS ANALYSIS MODELS

As described in the previous chapters, the net impact model has been designed to provide states with important information on the effectiveness of their JTPA programs. Although much could be learned from implementing only the net impact model, considerable additional information could be obtained by also implementing the process and gross impact evaluations described in other volumes in this series. That is, it is important to recognize that the net impact model complements the gross impact and process analysis models by providing information that is useful to both models and by drawing on information generated from these models. In this concluding chapter, we briefly indicate how the net impact model relates to the other models that have been developed to assist states and SDAs in better understanding the operations and impacts of their JTPA programs.

An SDA process analysis is a very important source of information for the net impact model. In fact, because of the inherent limitations of the nonexperimental approach to be used to estimate program net impacts, we regard an SDA process analysis as a necessary first step to a valid net impact analysis. That is, because the validity of the net impact results rests on the similarity of the participant and comparison groups selected, it is critical to understand the JTPA participation selection process, the factors that govern the assignment of participants to program activities, and differences in the content and recording of program activities across SDAs. Without an in-depth process analysis, one would be much more likely to obtain biased results, or have no information concerning the direction and magnitude of the likely biases, which could result in serious misinterpretations of the net impact findings. For example, as we have indicated in previous chapters, a process analysis will provide the following types of useful information for the net impact model:

- It will provide a detailed description of the criteria (explicit and implicit) used by SDAs and their subcontractors in screening JTPA applicants to choose individuals for program participation. As such, the process analysis will yield important insights into the extent/type of "creaming" that occurs and the likely differences that may exist between participants and comparison group members that are not possible to control for in the net impact model. This will be of considerable help in interpreting the net impact findings.

- The process analysis will also include a detailed description of the procedures followed in assigning participants to program activities (e.g., whether more advantaged participants are assigned to specific program activities, whether all participants are first assigned to JSA and only those who are not immediately placed are subsequently assigned to CT or OJT). This information will be useful in interpreting the results of the net impact analysis by program activity. That is, it will help in determining whether the estimated net impacts by program activity are likely to truly reflect the relative effectiveness of different activities, or merely represent the fact that more-advantaged individuals are assigned to certain activities, while less-advantaged individuals are assigned to other activities.

- The process analysis will also identify major differences in the content of program activities across SDAs, as well as differences in the ways in which similar program activities are recorded in the JTPA MIS. This information will be very useful input to developing meaningful and consistent measures of program activities across SDAs.

- Finally, the process analysis will be useful to the net impact model by identifying variables that should be included in the model. For example, it may identify SDA characteristics (e.g., service delivery strategies) that are quantifiable and that differ across SDAs so that they can be included in the model to test whether the net impact of JTPA significantly differs across these dimensions. In addition, the process analysis may be able to identify SDA characteristics that could play the role of instruments in the net impact analysis to correct for selection bias as discussed in Chapter 5.

Thus, information from the process analysis will be valuable in conducting the net impact analysis and in interpreting the findings.

In addition to benefiting from the SDA process analysis, it should be noted that the net impact model may also produce information that would be of interest to a process analysis. For example, the net impact model may indicate that after adjusting for differences in participant characteristics and local labor market conditions that the net impact of JTPA is considerably larger in some SDAs than in others. The process analysis could then examine in more detail what it is about the specific SDAs that may account for such differences in net impacts. If measures of specific SDA attributes that are potentially responsible for the different net impacts could be developed, these could be included in subsequent net impact models to determine whether they account for the different net impact estimates across SDAs. Such ongoing interaction between the process and net impact models highlights the complementary nature of the two models and should result in an improved understanding of the factors that affect program effectiveness.

The net impact and gross impact models are also quite complementary. Although both models are designed to address program-effectiveness questions, they differ in terms of the population subgroups of interest, the range of outcome measures of interest, and the types of comparisons being made. For example, as described in Chapter 2, the net impact model is limited to adults only and, because it relies exclusively on administrative data sources, there are relatively few outcome measures that can be examined and only a limited number of personal characteristics can be included in the model. On the other hand, the gross impact model includes youths as well as adults, an expanded set of labor market outcomes, and additional personal characteristics that can be included in the model. As such, the gross impact model can be used to answer certain effectiveness questions for youths served by JTPA and may also be able to provide information on the mechanisms through which JTPA affects adults' earnings and welfare dependency. That is, because of the additional outcomes that are available, a gross impact analysis may be able to provide some evidence on whether the earnings changes are likely due to changes in wage rates, changes in hours worked per week, or changes in weeks worked per year, although a comparison group is necessary to provide definitive evidence on these issues.

As the above discussion indicates, because the focus of each of the models is quite different, the information generated is very complementary. Taken together, the three models form a comprehensive analysis package that can provide much needed information on JTPA programs that will help states and SDAs meet their increased accountability responsibilities under the Act.

Finally, in addition to providing complementary information on different subgroups and outcome measures, information from the gross and net impact models may shed light on important methodological issues that affect the validity of analyses of program impacts in general, and that would be of considerable interest to the research community. For example, the gross impact model uses interview-reported earnings, whereas the net impact model relies on UI Wage Records to create measures of earnings. As discussed in the evaluation guide, there are advantages and disadvantages to both approaches. By implementing the gross impact model and by estimating gross impacts using the net impact design and with the same samples of participants and comparison group members, it would be possible to provide evidence on the extent to which the impact results are sensitive to the use of the different sources of earnings data. In addition, because the gross impact model has an expanded set of independent variables available, by implementing both models using the same analysis samples it would be possible to get some idea of whether the net impact estimates are sensitive to these omitted variables. Such comparisons would provide important information on the limitations of the different analyses and indicate other independent variables or outcome measures that would be important to collect for subsequent program analysis.